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ART. I.—*Report on Asiatic Cholera, as it appeared in New Albany Indiana, in the year* 1852. By ALBERT DE LESZCZYNSKI, M. D. of New Albany.

The steamer Midas, from New Orleans for Cincinnati, landed at our port in the night of the 5th of June, having on board, besides her cabin passengers, one hundred and seventy-five deck passengers. The Midas is one of the smaller boats. She had been on her voyage, up to her landing here, about fourteen days. During this time several of her deck passengers had sickened, and sixteen of them, after a short illness, died. On the 6th of June, at 11 o'clock, A. M., I was called upon by a passenger of this boat to see his infant child, which was then in a dying condition. It was attacked by hydrocephalus acutus. Meanwhile, several of our citizens, having heard a report that the cholera had broken out on the Midas, and that

several persons had died of it, as also the very deplorable condition of the yet remaining passengers, went on board to assist the needy, and waited upon me to see and assist the afflicted. I went and found the report but too true. The Asiatic Cholera was on board. Two children, a boy and a girl of respectively 11 and 8 years, were dying. Four others, three females and a man, were hurrying towards collapse, and a number of others were suffering with that malign diarrhea usually enumerated among the "premonitory symptoms."

As I afterwards learned, some of our Germano—American citizens—following the dictates of a kind heart, had induced a number of the passengers of the Midas to land and stay here, to avoid further exposure. Four moved into a private family, and twenty-eight of them into a two-story frame house, in the suburbs of our city.

On the 6th of June, at 10 o'clock, P. M., I was called to see the first patients of these immigrants, (of which it was my lot to treat professionally twenty-eight.) One of them was a man, the other a female. Both were attacked with cholera, and both were in collapse. What I understand by cholera, I shall describe under the head of *symptoms*, and only add that the house which had been taken possession of by the immigrants became, in a short lapse of time, the fruitful centre of contagion, whence the epidemic spread in various directions, slaying with unerring hand, all those who did not apply for timely aid. The entire number of cholera patients treated by me, up to the present time, is forty-nine.

Symptoms.—I shall here recite all symptoms. To give it a greater order, I shall divide the disease into three stages, (as indeed nature self-divides it,) and delineate the marked symptoms of each division.

Cholera Asiatica observes:

1. The stadium prodromorum—the stage of the forerunners, or that of the premonitory symptoms.

2. Stadium algidum, seu cyanoticum—that of the collapse, and

3. The stadium reactionis—the stage of reaction.

1. Stadium Prodromorum. *Symptoms.*—In some of the epidemics which, at different epochs, have visited large divisions of the earth, premonitory symptoms have been wanting to a great degree; in others they were of longer or shorter duration. *Here*, I observed, objectively and subjectively, the following: Great anxiety; pressure in the præcordial region; vertigo to a greater or lesser extent; slight, or sometimes stronger, headache; ringing in the ears; frequent sighing; dyspnoëa. The appetite is disturbed; the energy depressed; a feeling of weakness, mostly in the lower extremities; and a sensation of a peculiar lightness in the superior posterior part of the thorax. If the patient felt free of headache, the head felt remarkably clear, and the perceptive faculties seemed rather increased. The pulse was quick and easily compressible; the urine became reddish and scanty; the skin, in some instances dry, in others secreted, profusely, a clammy, even cold perspiration. Lastly appeared borborygmi, resembling the moving of gases and fluids, and finally diarrhea. It must not be supposed that all these symptoms took place gradually. Between the first and diarrhea, in some instances, hardly a few hours intervened. Sometimes diarrhea appeared without the slightest admonition. It generally lasted from three to six days but in some cases, hardly as many hours, before changing into the next stage. The diarrhea was not as painful as that accompanying dysentery or typhus; on the contrary, it always, as it were, relieved the patient, who, when it lasted any time, gradually became exhausted. The color of the stools in the commencement, generally, was of a dark green, sometimes light yellow, gradually changing into a lighter one; the quantity copious; the evacuated matter fœtid, but becoming less and less so, as the dis-

ease advanced, and when those flocky bodies were in a gradually increasing quantity admixed, which appear in the so-named rice-water discharges. The thirst was great; the tongue white, broad and pale—sometimes livid; the eyes sunk more or less, sometimes surrounded by a dark livid areola; the conjunctiva bulbi, in this stage, remarkably bloodless. Here I must mention the remarkable fact that all, previous to diarrhea, labored under constipation.

2. Stadium Toxicum, Algidum seu cyanoticum.—Among those forty-nine patients treated by me, there was not one where the cyanotic stage had appeared without, although sometimes only a very short, preceding ailment. Of the entire number, twelve cases were collapsed, twenty-six seriously affected, threatening collapsus, and eleven less dangerous. Ten of the collapsed cases complained for several days previous, (and previous to my attendance.) One was collapsed three, another seven hours after the appearance of the first symptoms.

Symptoms.—Vomiting and purging of rice-water-like, scentless matters, which continue incessantly, until at last exhausted nature ceases to evacuate. The eyes are sunk into the orbita, the conjuctiva bulbi et palpebrarum is highly injected, the rami of the blood-vessels form a net-work over the eye-ball. The head is hot, covered with a clammy perspiration; the tongue, cheeks, ears, are cold as those of a corpse; the expirations are likewise very cold; respiration very hurried; the pulse, at the commencement of this stage, small and fluttering; soon ceases to beat at the extremities—and even that of the carotids can shortly not be felt. The contractions of the heart are very feeble and intermitting; the respiratory murmur scarcely audible. The thorax is still warm; there is great pain in the epigastrium, like the burning of a coal. The patient complains of violent cramps, which in most cases commenced in the great toe, moving up the legs into the abdomen, when dysponëa takes place. The

skin, having lost its turgor, in most cases is covered with cold, clammy perspiration, and corrugated on the extremities and neck, but strongest on the fingers. The thirst is extreme; the patient continually calls for cold water, which, as soon as taken, is emitted. The papillæ of the tongue are shrunk, that organ self-covered with a slimy, ropy mucus, of the same repugnant smell as that of the slime evacuated by an emetic. The secretion of saliva decreases, and the patients complain of burning in the throat, which partially causes their insatiable thirst. The urinary secretion is entirely suppressed. The patients retained their mental faculties to the last, although some of them, in some cases, were impaired so much, that they seemed to lay in perfect apathy, out of which they could only be aroused by shaking, or very loud talking. This stage lasted with those patients, I observed, from eight to forty-eight hours.

3. Stadium Reactionis.—Although a reaction takes place in the first stage, I shall pass it, and only describe the reaction I observed after collapsus. Only three of my patients died during the latter; in nine, reaction took place, but only three recovered entirely.

Symptoms.—The patients became easier; the radial pulse returned; evacuations ceased either entirely, or became less numerous, and strong congestions took place towards head and chest; the former, during advance of reaction, became hot, the sunken eyes elevated, the face alternately flushed and pale. Perspiration appeared, but the extremities remained cold with those who died, and became warm with those recovering. All lay in a comatose condition; the eyes were still much injected; the tongue, when protruded, broad, thick, dry, and trembling. Urine was voided, its color reddish brown, but became soon limpid with those who survived, containing a surplus of urea, (as uric acid) The gums and lips were covered

with sordes similar to that in typhus. This period lasted from two to six days, and with one, who recovered, above it.

THERAPEUTICS.—Various were the remedies I employed. The strongest excitantia served in some cases, in others, cerebro-spinantia and astringentia proved themselves of value. In the first stage, plumb. aceticum cum opio; or, if an idiosyncrasy against the latter existed, or as a precaution to children, I gave in its stead hyoscyamus. This, rest, avoidance of cold water and solid food, only permitting rice or barley-water, in very small quantities, soon restored the patients, and the primary changed into a secondary disease only in a few cases. But in every case where this occurred the patient was at fault. During collapse, the strongest stimulants were given. Camphor and aq. ammon. causticæ, diluted with wine, saved one case. I gave equal parts of aq. ammon. and tinct. camphor. ten drops half-hourly, in one ounce of Rhenish wine, and good chicken broth hourly. But, above all, I value the exhibition of Chloroform, from seventy to one hundred drops, in milk or mucilage, repeated every quarter or half hour, until vomiting ceases, or sleep supervenes. This remedy I used only in the latter part of our epidemic, after I had tested its unsurpassed efficacy in a violent case of colica pictonum, which strongly threatened to combine with gastritis, yielding neither to bleeding, opium, alum, croton oil, tobacco, nor cold-water injections—was subdued in a few hours, after the subsequent internal exhibition of chloroform.

After the first effects of this substance: cessation of vomiting, of cramps, sleep, and, in some instances, perspiration—opium, gr. ¼–½, with plumb. acetic. grs. ii–iii, and pulv. camphor, gr. ½–i, were of excellent service. These latter remedies I gave until the entire suppression of the alvine evacuations, repeating it every two hours; after which I prescribed quinine sulph., grs. ii–iii, cum

opio, gr. ¼–½ pro dosis, repeating it every four or five hours. During the stadium of narcotization—vulgo the typhoid—I gave wine and the mineral acids, of which, particularly acid. sulphur. and alum were of the greatest import.

There were twenty-six cases threatening to run into collapse; chloroform invariably arrested their fatal progress. It proved of the greatest service, when even mosch. oriental. arnica, valerian, and ether, had been hopelessly employed. In all cases of the first and second stage, where the extremities were cold, applications of hot bricks to the feet, bottles filled with hot water surrounding the body, and sinapisms from the chest downwards, were very serviceable. In some cases I employed frictions and fomentations, with hot liquor, as recommended, the blister and the endermic exhibition of morphine on the epigastrium, but with very little success. The use of the lancet was, in every case, even in the stadium reactionis, out of the question. Regarding the local abstraction of blood, I must mention that, although it seemed indicated in one case of reaction, where I applied four cups to the nape of the neck, it proved worse than useless. Emetics of ipecacuanha were, in the first stage, of *some* service. The employment of calomel, either pure or combined with opium, proved always injurious, as also that of mild cathartics in general, even as late as the second day of convalescence, which, in a few cases, even provoked relapse. All gastric symptoms generally subsided after the employment of quinine with opium, as before mentioned; and only when this had been given for perhaps three or four days without producing evacuation, a slight purgative, such as pulv. rhei, grs. viii–x cum hydrarg. mur. m., grs i–iii, could safely be given. All the remedies I have mentioned have received a full and fair trial. Only, those I observed proving in-

jurious, I discontinued immediately, always having before my eyes the wished for end: the saving of human life.

After having stated the remedies which, in my practice, proved *most* successful, we shall pass a small critical review on a few of them, as regards their physiological effects, in order to judge of their respective merits, and the justness of their application.

Chloroform, Plumbi Acetas.—Of the other remedies mentioned I shall not treat, as they, at various times, have been fully discussed. Neither mean I to intimate that the combination of acetate of lead and opium has been only used by myself, as I am fully aware that many physicians employ it, and also that the great Dupuytren warmly advocated it.

Chloroform—Although its relation to the excitantia is undeniable, it belongs not solely under that head. There can be no doubt, from its external and internal application, that it more justly belongs to the cerebro-spinantia,—of which the narcotic acria also form an order. When applied externally, and undiluted, it causes pain, redness, and blistering, wherefore it must be an acrid. But as it causes disturbances of the cerebrum, as narcosis (anæsthesia) of the motory apparatus, (contractions and contortions of the muscles, especially of those of the face, particularly if given internally,) of the cerebellum and spinal marrow, (convulsions,) it follows that it is a cerebro-spinantium, and as such acts as the most powerful sedative ever known.

When inhaled, or exhibited internally, it at first causes the pulse to increase slightly in frequency, but, presently, as soon as narcotization commences, the circulation becomes slower, the pulse fuller and stronger. Here the stimulating powers cease, and the sedative effects commence. Its actions than extend from the cerebrum to the cerebellum, and elongated marrow to the spinal cord, and necessarily to the peripheric nerves. But, over the

great sympathetic nerve and its branches, it has not only no sedative, but a strong stimulating effect, as is thoroughly proved by the violent contractions of the uterus in labor, when this agent is given. Here labor rapidly progresses, although the parturient woman is entirely under its anæsthetic influence.

We shall now pass to review the physiological actions of the *stimulants*, and their changes in the system. I shall not give my individual opinion of processes they undergo and produce, but those of men of acknowledged authority, and add only, that close observation and experiment induce me to adhere to their doctrines.

All stimulants influence, more or less, the functions of the nervous centres,—the substance of the nerves. *Their local actions* are the more intense, the less their volatility and the stronger their affinity to the constituents of the organic structures and fluids. They act upon the mucous membranes and the external integuments, increasing in strength, the finer the epidermis covering them. The primary sensation will be that of heat—even intense burning—and only the most volatile of this class will cause a sensation of cold, a transient decrease of temperature, in consequence of their rapid evaporation. Parallel with the change of the sensible nerves of the dermis, appears an action upon the walls of the blood-vessels and their diameter. They become expanded, and hyperæmia, even congestion takes place in those parts acted upon bythe stimulant. The blood vessels become narrower, the parts acted upon pale, anæmia takes place, which only, after a while, changes into hyperæmia. The local action of stimulants justifies the proposition, that there is a very slender difference between them and the acriä. Stimulants as æther, alcohol, and also chloroform, coagulate protein bodies.

Internally many of the stimulants cause contraction of the muscles of the intestines. Some of them act as

mild astringents, and frequently they increase the contractility of the biliary canals, the excretion of bile, and *it seems* as if they had the power to increase the secretion of this matter, by acting chemically upon the blood. All volatile stimulants are rapidly absorbed, and taken up into the circulation, without undergoing peculiar alterations, as other matters will; most so with alcohol, æther, volatile oils, and chloroform. In consequence of this, when taken into circulation they will very rapidly act upon the nervous centres, until acted upon by the oxygen, when they are excreted. Their excretion is nearly as rapid as their absorption. The lungs, kidneys, and perspiratory organs will return that portion of the stimulants which remained unaltered, as proved by the urine, perspiration and exhalation.

Lastly, all stimulants produce a change of function of the *nervous centres* and *circulation*. The former is announced by an exalted condition of the Psyche. Mental depression, sorrow, sleep, apathy, disappear, and give room to a joyful hilarity and contentment. The latter is observable by the stronger contractions of the heart, the fuller, vigorous pulse, and heightened color. Simultaneously we observe an increase of heat. The cause of this I should seek in the abnormally increased supply of carbon, which necessarily asks for an adequate supply of oxygen, which latter is admitted by *accelerated respiration*. The latter and rapid oxydation will elucidate the cause for accelerated circulation and calefaction. How much this, likewise, may influence the nervous centres, and how much their functions, I am not prepared to say.

The ultimate effect of stimulants is a physical and psychical depression. But that this should be only the consequence of the preceding excitement, I must doubt. Should this be so, then excitement would continue as long as the stimulant is retained within circulation. But this is not the case. Although excitement has long given away

to depression, the existence of stimuli within the body is proved by their exhalation per lungs. This is the case particularly with æther and alcoholic substances. There must be another cause for depression. According to my view, the blood has undergone a chemical change, a narcotization. This hypothesis is substantiated by the fact that *the fumes of liquor still are exhaled, although a person may have been intoxicated for more than twelve hours!* If all parts of the introduced stimulus had been expelled, how could the fumes still be observed? Here another fact strikes us: the purer the alcoholic substances used have been, the shorter will be narcotization, provided the quantity has been the same.

A short recapitulation and application of the just said, will elucidate the virtues of chloroform. It acts upon the elasticity of the coats of the blood-vessels and capillaries, on account of its energetic *antispasmodic* properties. It increases the mucous membranes, so much so, that even hyperæmia may be caused by it. It coagulates protein bodies, produces contractions of the muscles of the intestines, and promotes the excretion of bile, (as proved by vomiting after the exhibition of a large dose.) What kind of chemical changes it produces upon the blood, and if it really causes increased secretion of bile from it, must yet be decided. Its action upon the nervous centres are well enough known. It exhilerates, soothes and produces a temporary irritable and paralytical torpidity (anæsthesia) of the voluntary apparatus, but an increased action of the ganglionic nerves, and of the organs they enter.

It is best given in milk or mucilages. The dose is from 25 to 75 or 100 drops, repeated every quarter or half hour, as circumstances require. It even has been given with good success in colic to 200 drops pro dosis, and the dose repeated. This I state upon the authority of Dr. Clapp, Sen., of this city, which shows that the evil

effects of chloroform are not so great as some physicians maintain. But the exhibition of this, like every other article of the materia medica, is subjected to certain laws, indicated or contra-indicated. It is *indicated* in most spasmodic or neuralgic diseases or conditions of the system, where other remedies may have failed, or where an immediate effect is peremptorily called for. I, on my part, should give it in every case where a safe stimulant and antispasmodic is indicated, and only should find it

Contraindicated—1. In a *plethoric subject*, or inflammatory disease, as inflammatory colic, where, previous to its administration a vein is to be opened, when it safely may be given.—2. In a *cachectic subject*, or one laboring under a dyscrasia. In the first case the stimulating powers might be too great, causing apoplexy; in the second, the chemical action of the remedy might decompose the already diseased blood to such a degree, that it becomes entirely unfit for nutrition, when death must ensue. We know the baneful influence of alcoholic substances on cachectic individuals. This acts in a similar manner.—Although the modus operandi of the substance upon the blood is not perfectly understood, its *effects* we name narcotization.

Plumbi acetas.—All soluble preparations and oxydes of lead when brought into the stomach, are acted upon by the secretion of the latter, and intestines, and enter the circulation. They unite with the albumen, but not with the fibrine or hæmatosin of the blood, (Cozzi, 1844.) Lead, when once taken into circulation, acts particularly upon the secretory processes, the membranes and the spinal marrow, with its peripheric continuations and terminations. The motory and trophical nerves are most affected by it. The secretion of saliva, urine, fœcal matters, the perspiration, in short, most all, se.—and excretions are impaired, and only in rare cases we observe salivation, (previous to which, on the gums, the so named

"blue line.") In minute and not too long continued doses, lead will not cause important alterations of function of the nerves, but when continued for a very long period, or when given as oxyde, or salts, in an over dose (℥ss—℥i or more), or if persons for a long time are exposed to its influence, then the functions of the nerves, ergo their substance is altered (as proved by post mort. sect.) In the stage of consecutive intoxication (as Tanquerel calls it), we observe hyperæsthesis, spasms of diverse peripheric portions of nerves, vertigo, headache, algia, even delirium and coma. This proves the affection of the ganglia, the spinal marrow and brain, the sources of the trophical, sensible, and motory nervous fibres.

When the nervous centres are acted upon by a powerful stimulant as Chloroform, when those alterations ascribed to it have been produced, it becomes necessary to check the profuse secretions, whilst simultaneously we must be thoughtful to stimulate the nervous system to continued action. This will be effected by plumbi acetas with opium. Both are cerebro-spinants, although differing materially in their individual effect. Opium given by itself I found insecure, not so the acetate of lead, which, when given alone, acts as a cerebro-spinal stimulant and astringent. As remarked before, where opium should be contra-indicated, or in children, hyoscyamus will fill its place. Of the other remedies used, I shall not speak, as any materia medica will serve this purpose. I should not have spoken so lengthily had not justice to the remedies demanded it. Although perhaps I may be mistaken, I think it wrong in any author who only enumerates his remedies used in a certain disease, without showing his reasons for their administration.

In writing this essay I have not adopted the general rules. My motives in acting contrary to these were, to present to the reader first, the clinical, after which I shall pass to the theoretical part. To comply with this

view I will now give the results of (I am sorry to say) the only and rather imperfect *autopsy* I held. The subject of this examination was Henry Schmidt, a German immigrant, aged 39 or 40 years, of robust, plethoric constitution. Autopsy six hours after death.

Head and spine I have not examined, as I had neither the necessary instruments with me, nor time to do so.

Chest.—Trachea and bronchi slightly injected, of a sticky feel, as indeed were all membranes of the chest; the pleura injected, and the right heart filled with a black, tar-like blood, the former not crepitating when incised. The diaphragm injected.

Abdomen.—The liver soft, full of blood of the quality before stated. A *remarkable fact* must be mentioned here. Upon the convex side of the liver, about in the angle formed by two lines, one horizontal, drawn from the incisura interlobularis, continued five inches,—the second, vertical, drawn from the superior posterior margin, continued two and a half inches, I found a *gangrenous patch*, of the size of a ten cent piece; and beside it another of the size of a pea. Depth of gangrenescence, one inch. This corresponded to a pain in the right hypochondrium, complained of by this and other patients. But as I had no other chance of a post mortem examination, I do not know whether gangrene here was *accidental* or *essential*; my belief turns toward the latter conjecture.

The gall-bladder was full, containing ℥iv ℥v of dark green bile.

The stomach, enormously distended, contained about three quarts, of which two were rice-water-like fluids, containing the specific flocculi, and the third, meat and vegetable substances, which, eaten twenty-four hours previous, although excessive vomiting had taken place, never had been evacuated, nor acted upon by the gastric juice, so that it was easy to distinguish the species. The coats were excessively thin, the mucous membrane in

several places *injected*. It was easily scraped off with the handle of the scalpel. *The pancreas* was contracted, anæmic grayish-white, *the spleen* of normal texture and size, filled with tar-like blood; the smaller intestines distended with the same fluid and gas found in the stomach —in several places injected; the large intestines, distended with the same fluid, were otherwise normal. They contained a number of ascarides lumbricoides. I forgot to mention that some of the patients who died evacuated a great number of these worms. *The kidneys* were bloodless, the *bladder* contracted to the size of a large walnut.

ÆTIOLOGY.—Cholera Asiatica, as the name tells, a child of Asia, has been transplanted at various times from its place of birth to Europe, thence to our continent. Its genesis, although enshrouded much in darkness, has, by various observers, been attributed to *malaria*. Although *malaria* is a very vague idea, so much must be certain, that one, causing cholera, must be *sui generis*. Every plant bears its own seeds, and every specific seed generates its specific plant. When the lower division of the organic kingdom follows certain prescribed rules, why, I ask, should not the higher obey like ones? Certain causes produce certain chemical changes in the vegetable kingdom, according to irrevocable laws and specific causes must produce specific results in the animal kingdom, according to like irrevocable laws. Should the highest species of animal, man, only be an exception? As much as his growth, perfection, and decay are carried on by certain rules, so much must be his diseases. As soon as the normal laws conditioning health are infringed by a specific noxiousness entering the organism, so soon will another set of chemical actions be instituted, which, derogatory to growth, perfection, or preservation of the individual organism, will either cause a greater or lesser destruction of the individual fabric—*death or imperfect convalescence*—or the specific noxiousness has been consumed

in the process, or annihilated by art, when the primary chemical processes will be supreme again—health. The revelation of the ætiology of physiological and pathological processes, is the aim of the true physician.

If cholera is a specific disease—and no one doubts it—then it must necessarily have a specific cause,—it must be of a contagious nature. This latter has been denied. Every practitioner of the South knows that intermittens perniciosa—vulgo, congestive chills—appears at certain times, endemically and epidemically,—so that the question involuntarily arises in one's mind, can it be contagious? Of the law regulating the genesis of epidemics, endemics, and contagion, anon. Professor Fuchs, of Gœttingen, mentions in his manual of Nosology and Therapeutics, facts which strongly speak for the contagiousness of cholera. Some say it is not contagious, but infectious; some, again, it is portable. What is the difference between *contagium* and *infectio?* Infectio literally means, *the introduction of a mass—of an agent;* whilst *contagium* signifies, *an infection communicated by actual contact*—from *con* and *tango.* The signification of "portable," every one knows, and logic teaches, *when a portable matter can cause a specific disease,* IT MUST BE A CONTAGIUM. I hope that I shall prove that cholera, at least under certain circumstances, can be contagious.

As I mentioned in the introduction, four persons took lodgings in a private family, whilst the other twenty-eight moved into an empty house. H. S., of the first party, died. Over his body I held the post mortem examination. Two men, his host and another, assisted me. Eight days after, his host, Mr. W——t, and myself, and fourteen days after, the other man, were attacked. This man died. He lived about one mile from the scene of action. Nine days after his decease one of his children* died likewise. On the day of H. S.'s death—the first victim—his oldest

Cases marked thus: (*) signifies, not treated by me.

daughter was attacked. The next day a Mr. H——l took her, from charity, into his house, which was situated about one and a half miles from where she had been taken sick. At this time I did not as yet believe in the contagious nature of the disease. Seven days after, Mr. H. was a corpse. A woman, a relative of his, living about one and a half miles from him, in the country, washed the clothing he died in. Eight days after *she* was dead.* Five days more, and two children died in the house this woman had lived in.* A neighboring lady, visiting Mr. H., brought the cholera home; she and two more of her family were attacked. Let us return to the first mentioned house of Mr. W——t. His wife was taken ill with it. A brother visiting this family, and residing over one mile from them, brought the disease home. He and five of this family were attacked; the first one of those attacked died. I brought the cholera into my own house. All my colleagues of this place could adduce a number of cases of like import. We are all of the opinion *that cholera can be, and, at all events, this time has been contagious.* When the contagiousness of our epidemic cannot be denied, the question arises: Why has not cholera been contagious in previous epidemics? I only shall speak of what I have seen, not of what I heard. In 1849 I observed this disease in St. Louis and New Orleans. The latter port was the first affected, whence it spread to many landing places of the Mississippi, until arriving at St. Louis. It was in January of that year there appeared the first cases, *imported* by travelers from New Orleans. Cholera was likewise *imported* from Europe—it was not autochthon. Physicians, persons and nurses attending the sick, were attacked, and many even died of its ravages. Does this, too, speak *against* its contagious nature? Some may say that not all persons attending the sick were attacked, and that this fact proved its *non-contagiousness.* But which *acknowledged* contagion can extend its influence over every person?—How many cases of the most malignant form of variola

have I attended, and how many victims of this loathsome foe dissected, without being injured by it myself! This proves that cholera, like any other malignant disease, observes the law of "predisposition." Not only this, but other causes may have prevented cholera from becoming in one place more than endemic, or only appear sporadically, although transplanted by direct communication, whilst in the place it emigrated from, it raged as an epidemic. They may be double. To answer the first, I shall quote here part of an article I wrote in 1850.

As nature never transgresses her laws, cholera must have its germ. *I do not doubt but this ens proprium associates with malaria, but doubt that malaria, without the germ of cholera can become cholera proper.* Therefore yellow fever, and all the host of specific diseases malaria is charged with producing, must have their respective germs, which undoubtedly enter into alliance with malaria. Nay, more: I firmly believe that malaria is the nucleus of these different germs; that it only stands in such a relationship to them as the *albumen* to the *vitellum* of the egg. *Ergo, to produce malaria, sulphates are required. Now, if a vicinity or place is not supplied with these bodies, (sulphates of lime and of magnesia,) malaria cannot arise.* Let us suppose malaria and sulphuretted hydrogen the immediate causes of cholera, how could it be possible for these to produce such a diversity of disease? I acknowledge it possible that different degrees, but only of one and the same disease, could be produced, according to the greater or smaller quantity generated. But there are places which contain either more or less of sulphates, and the disease, as an individual hardly varies in intensity. It is possible that when the germ of cholera is introduced, some places are perfectly exempted, whilst others are nearly devastated. Again, at a time of recurrence of the scourge, the first place may have acquired the conditions necessary, whilst the other has not. The second cause is a want of predisposed persons to increase the disease to

an epidemic, when the contagion necessarily becomes extinct.—Now we inquire about the nature of contagion.—First, what is contagion? This name we apply to all those noxious products generated in the animal organism, which, when transplanted into an organism of as high a standing as that in which this deleterious matter has been produced, will cause absolutely the same organic changes—*disease*—as that which caused its genesis. We shall now pass to the genesis of *epidemics*, from which we will deduce the genesis of *contagion*.

When the atmospherical and telluric noxious influences produce upon the organism so great a chemical change as to cause a disturbance of functions of the body, then we call this abnormal state disease. The abnormal, like the normal state, has its products and educts. When the functions are changed, the organic structure must change likewise; ergo, the products and educts of the body likewise. The more intense the organic and functional changes are, the greater abnormity the products will exhibit. When there occurs one or more cases of the same lesions, the same external symptoms,—also of the same disease, we call them "spontaneous." Should those telluric or noxious influences extend over a larger range of country, causing disease, we say it is endemic; but if they extend over still larger districts, attacking a very great number of individuals, we say the disease has become an epidemic. When during the prevalence of an endemical or epidemical disease, many persons attacked by it are enclosed in relatively small rooms, to which not enough fresh atmospherical air can be admitted, then these rooms will not be filled with carbonic acid, but *with animal products of exhalation*, and perhaps with those of the voided excretions, which will now putrify, and not only produce the original disease, but by far a more virulent one, causing, in some, dyscrasy of the blood, but in all *affecting the nervous centres*. This new product we call *contagium*, and

the manner in which it originated its *genesis.*—It is not necessary that contagium be generated only in enclosed rooms. If many individuals under similar conditions are attacked in a low malarial region, where change of atmosphere, on account of its geographical position, is impossible or difficult—as in valleys to which the wind has no access—and where, perhaps, yet planetary influences, may exist (as the hot rays of the sun,) there contagion will likewise be generated, and will be the more lasting, as it finds a ready nucleus in malaria. This I conceive to be the process in which the contagium of wide spread epidemics is caused.

In this manner change the following and other sporadic, endemic, and epidemic diseases, into *contagious epidemics*, as, yellow fever, scarlatina, variola, the plague, tussis convulsivus, *Cholera Asiatica.* A contagion once generated acts entirely different, on account of its more virulent nature, and when carried to other places, appears there as contagious disease, not passing through the same process described, nor needing those peculiar favorable conditions as are required to its production. It appears as contagium, and will exist as such, as long as circumstances mentioned before do not exclude it.—The next question to be answered will be: Of what nature is Cholera Asiatica, or to what genus of disease does it belong? My answer is: *It is an intermittens, holding the same relation to the so-named organic nervous system, as intermittens perniciosa holds to the cerebro-spinal system,—in other words, a primary localization of the typhoid process upon the ganglionic nerves, which becomes transmitted secondarily to the central nervous organs.* L. W. Sachs, of Kœnigsberg, holds it likewise for an intermittens,—but I am not acquainted with his theory. My reasons for the identity of both diseases rest entirely upon physiological and pathological resemblances, as also upon the similarity of localities in which both originate, and the frequent change from one into the other. Let us draw a comparison between them:

PLACE OF NATIVITY.

Intermittens.	*Cholera.*
Low marshy grounds on the banks of rivers and swampy places, where malaria can originate.	Low marshy grounds, in the deltoid countries, through which the Ganges passes, and those of Bengal, Ceylon—all malarious districts.

CAUSE OF GENESIS:

MALARIA.

MODE OF ACTION UPON THE SYSTEM:

INTOXICATION OF THE BLOOD.

CIRCULATION.

The blood is removed from the peripheric to the central parts. The contractions of the heart are numerically increased.	The blood is removed from the peripheric to the central parts. The contractions of the heart are augmented.
During the cold stage, the temperature of the lower extremities is sunken, the skin of the fingers corrugated, lips, and sometimes the extremities of a bluish color.	The temperature of the upper and lower extremities is sunken, except that of the thorax and abdomen.—(Symptoms given before.)

NERVOUS SYSTEM.

The seat of this disease is that part of the peripheric nervous system, which enters into the closest connexion with the vascular.	The seat of this disease are the ganglionic nerves, following primarily their nearest nervous plexuses, and attacking, secondarily, through their spinal fibres, the spinal and cerebral organs.
In the ganglia-intermittens we observe a strongly marked cold stage, whilst in the cerebral intermittens it exists only slightly, or not at all. Neither appear in the latter perfect crisis per perspiration and urine.	Here we have the highest expression of the cold stage. When terminating favorably, crisis by skin and urine takes place.
As well as cerebral—may change into ganglia—intermittens, the latter may change into the former. A cerebral intermittens may originally attack the trigeminus, from hence passes to the vagus—where it causes disturbance of respiration—following its ramifications until it reaches the ganglionic nerves, and a ganglionic intermittens, may, in a similar but reversed manner, change into a cerebral intermittens.	Cholera commences in the ganglionic nerves, attacks the abdominal plexus, thence follows the relations of the vagus, causing disturbance of respiration, dyspnœa, and finally settles upon the cerebral organs.
Intermittens may locate upon the central parts of the abdominal nervous system, when it will cause violent emesis and diarrhea, which is physiologically explained by the intimate relation between the ganglionic system, and the abdominal mucous membranes.	Here we have violent emesis and diarrhea, on account of the localization of the typhoid process upon the ganglionic system, and the relations of the latter to the abdominal mucous membranes.
Intermittens is confined to the human species.	Cholera is confined to the human species.
This disease, at a time when cholera is prevailing, may change into cholera. It likewise may change into enterotyphus.	Cholera will change into intermittens, and in some instances has changed into enterotyphus.

NERVOUS SYSTEM—*Continued.*

Intermittens.	*Cholera.*
During the cold stage it may end by paralysis, causing hydrops, one kind of which takes place suddenly, by exhaustion of the abdominal nerves.	Cholera in the first stage ends by paralysis. Here accumulation of water takes place, which is evacuated per mouth and anus.
In some instances we observe a papular eruption—as auxiliary crisis—in the mouth, nose, the mucous membranes, and even over the entire skin, which by the older physicians has been named "critical itch."	In some instances we meet with an eruption resembling roseola, wherefore it has been named "roseola cholaica," which in every case is a good symptom. I met with it in two cases; both were women.

I shall add a few physiological facts, as a proof that cholera attacks, primarily, the ganglionic nerves.

Vagus.—This nerve—not properly belonging to the ganglionic, on account of its intimate connection with this system, I speak about it here—sends many rami to the lungs and the heart. When irritated, respiration becomes difficult and hurried, and the circulation more rapid. When the contractions of the heart of just killed mammalia have ceased, they may frequently be caused again by pressing the cervical portion of the vagus. E. Weber and Budge have most clearly proved the influence of the vagus upon the heart, by their experiments with the magnetic-electrical rotatory apparatus. When G. Valentin mechanically irritated the cervical portion of the vagus in recently killed dogs, cats, rabits, or horses, he observed distinct motions of the stomach. The latter sometimes became contracted, sometimes forming an incisura from the greater to the lesser curvature, and at other times performing wave-like motions from the cardia towards the pylorus. Anatomy shows that branches of the vagus connect with the solar and hepatic plexuses, which fact elucidates the influence of the cervical portion of this nerve upon the large and small intestines.

Sympathicus.—Valentin observed that the cervical portions of the vagus and sympathicus, the inferior cervical ganglion, the rami cardiaci, and cardiac plexus may either produce or alter the contractility of the different portions of the heart. The stomach may be influenced by the

sympathicus into four kinds of contractions, one of which is a peristaltic motion of principally the pyloric portion. When Volkman applied the magneto-electro-motor to the thoracical part of the sympathicus proper, violent peristaltic motions of the stomach and intestines ensued. (Experiments made on an asphyxiated cat.) When the same applied the wires to the ramus splanchnicus major, before it enters the cœliac plexus, strong contractions of the stomach were produced. John Muller, Kuerschner, and other physiologists, have observed that a mechanical or chemical irritation of the plexus cœliacus will produce violent peristaltic motions of the smaller intestines. The lumbar and sacral parts of the sympathicus produce strong actions of the larger intestines. M. Schiff, in his "De vi motoria baseos encephali inquisitiones experimentales," says that when he had extirpated several of the posterior abdominal ganglia of the sympathicus of a frog, he found the peritoneum and the intestines infiltrated, and the latter and the abdominal cavity filled with effused fluids. The present state of our knowledge of the physiology of the nerves is such as to leave plenty of room for inquiry. When, therefore, many points, as regards the nervous pathological influence in cholera, remain shadowed, we have a right to judge by analogy, from the results of experiments instituted upon nerves in their physiological state, on the pathological condition of these identical nerves by the resemblance of their functional disturbances. Footing upon this right, I judge that *Cholera is a primary disease of the ganglia.* When one pathological process—disease—whose nature is disputed, in its symptoms resembles another, of fixed form and undisputed nature, more than any other, we are justified to classify it with the latter process. Upon this ground I classify cholera with *intermittens.* I have forgotten to state, in the proper place, that I observed *one case of cholera change into intermittens perniciosa, and two cases of the latter become cholera.*

Since I wrote this I have read the report of the Committee on Practical Medicine and Epidemics, (of the American Medical Association for 1852,) on which I shall take the liberty to make a few remarks.

As regards the different curative means which proved, in the hands of many, remarkably successful, whilst others complain of the reverse, it is here the place to say that cholera, like every other disease, differs widely in its nature. It may be simple erethic, inflammatory, or torpid. The *erethic form* calls for the least interference—nature needs only to be guided carefully—whilst the *inflammatory form* requires an energetic treatment. Here antiphlogistics, with which men of high authority have had brilliant success, are in their proper place. The third, the *torpid form*, calls for an entirely different course. Here the antiphlogistic regimen would be downright murder. The cerebro-spinants and roborants are in their proper place. The sinking powers must be reanimated, the apathic nerves called into a new action. The difference in the nature of this disease depends not only upon the cholera-virus as regards quantity and quality, but also upon the subject attacked, upon epidemical and terrestrial conditions. The disregard, or the malperception of these important causes, has been the reason for many failures of remedies, which others, at certain times, found unsurpassable. Here I must remark that those remedies which I found beneficial, may be to others, at later times, perfectly useless. Only the nicest discrimination of local and individual constitution, and of that of the then reigning genius epidemicus morborum, will prevent this. The latter must always be considered, as it is of immense import and regulates the selection of our therapeutical agents. All *our* local diseases partake this year, more or less, of the so named "nervous" type.

Saline treatment.—Although some hospital reporters may praise it, I do not think the method upon which it is

founded a perfectly sound one. The late Prof. Canstatt, of Erlangen, states that experiments made by Dr. Vanderlinden, in the cholera hospital of Brussels, after the method of Stevens, proved not only unsuccessful, but that they seemed to provoke collapsus.

Emetics.—In gastric complications, particularly in the erethic form, they are of a salutary effect, but I think not advisable in *every case.* Their chief eclat, after emesis, consists in their general effects. Hufeland says: "These are due to sympathies, depending upon the gastric nerves. The connexion of these nerves with all other parts of the body, gives great importance to these general effects, and is the medium through which vomits affect every system of the economy."

As regards the contagiousness or non-contagiousness of cholera in St. Louis in 1849, I must say that I saw at least *one* physician of the St. Louis Hospital, intimately known to Dr. McP., attacked with premonitory symptoms. This was in January.

My intentions in writing this essay were to give to the profession facts as they appeared here. When I permitted myself to reason, I must be excused, as the import of the principles involved is strong enough to lead astray.—In some instances I have been guilty of repetition, but only where it became necessary to prove or elucidate by it other facts.

NEW ALBANY, IA., Sept. 15, 1852.

ART. II.—*A Remarkable Case of Reflex Nervous Action.* By J. W. BRIGHT, M. D., Louisville, Ky.

That nervous irritation in one part of the human system, by whatever cause it may have been excited, may be transferred to another part, or organ, is an occurrence so common that every practising physician is fami-

liar with it. For instance, the irritation of a calculus in the bladder will give rise to a pain in the glans penis, or pains in the thighs. Now those sensations are not produced by a direct communication of sensation through the nervous cords, but the physical changes excited by the first, are reflected in the second. Again, when the stomach is affected by the action of a strong acid upon its coats, there is often a severe pain felt in the supra-orbital nerve. An antacid taken into the stomach quickly relieves this pain. A lump of ice swallowed suddenly will often produce a similar effect in the supra orbital nerves, and a draft of water warmer than the ice will as soon relieve it. My object, however, in this short notice, is to give an account of a remarkable case of reflex nervous action which I attended a short time since.

I was called, September 8th, 1852, to see Mrs. B——, about six months and a half advanced in her first pregnancy, a very delicate lady. She had been attacked the day before with a chill, followed by fever of the usual intermitting form, which was, by the use of the ordinary remedies, removed in two or three days. About twenty-four hours after all the symptoms of fever had been removed, pains set up in the uterus, simulating those of labor. I saw her in five or six hours after these pains commenced, her friends supposing that she was in labor. Her pains came on regularly, and were of considerable strength. On examination of the os uteri I found the pain was of spasmodic character, and not genuine labor pains—with nothing of the expulsive character attending such pains. An anodyne was ordered, (morphine and tinct. valerian,) which soon suspended them in the uterus. But in a very short time the spasmodic action was transferred to the diaphragm, and hiccup set in, which became very troublesome, and resisted all the usual remedies for twenty-four hours, till finally it yielded to a preparation of creosote, sulph. æther and laudanum.

But notwithstanding she was under the influence of the above medicine, as soon as the hiccup began to subside the pain returned in the uterus, and by the time the hiccup was entirely relieved, the uterine pains were as severe as at first. I allowed them to continue for eight or ten hours, but no evidences of delivery appeared. The os uteri remained firm, and the head of the child made no descent. I then suspended the uterine pains once more by the use of McMunn's elixir of opium. Sixty drops had the effect, and she immediately commenced hiccuping again; but after a few spasmodic actions of the diaphragm the action passed to the brain, or membranes of the brain, and she fell into a profound stupor. In this state her pulse was one hundred and five in a minute, soft and compressible; her breathing only nine times in a minute. She could be aroused by shaking and talking loud to her, but would instantly fall into a deep stupor again. The pupil of the eye at this time was very much contracted, and not sensible to light. There was frequent knitting of the eyebrows. She could talk and swallow when aroused, but the pulse was too soft to bear the lancet. I ordered an active cathartic, to be followed by an enema. This operated well, but no relief was obtained. I then ordered strong cataplasms of mustard to be applied over the stomach and extremities, for they were too cool, and gave a full dose of calomel, and again met it by an enema, which also acted well. No relief to the brain. I then resolved to excite the uterus, and as soon as I effected it, and the pain was set up there, and labor commenced, she began to arouse and talk, and look about the room. In six hours from the time the uterus was again in active operation the child was born, and she was fully awake, and perfectly conscious of every thing that was passing.

Thus, it will be remarked, forty-eight hours passed before the uterus was made to act, and her brain relieved.

The child lived about twenty-four hours. The mother recovered without any difficulty. The remarkable character of this case has induced me to give it to the profession. The treatment is nothing more than any experienced member of the profession would have adopted under such circumstances. It is the reflex action of the nervous system shown in this case to which I wish to direct the attention of the profession. I leave it without comment. Although I have practised medicine for nearly forty years, I never saw exactly such a case before. I am induced to believe that they are rare, and, when they do occur, we need not expect relief until delivery is accomplished.

LOUISVILLE, October, 1852.

ART. III.—*A Case of Encysted Tumor Extirpated.* By. M. J, BRAY, of Evansville, Ia.

Mr. James Smith, residing in the country, came to Evansville to have a large tumor removed from the neck. He had, naturally, a good constitution, but at this time, and for two or three months previously, he had suffered from the irritable effects of the tumor. He was somewhat emaciated and reduced in strength, and had a slight fever every night, which disturbed his rest.

He was about forty years of age. The tumor made its appearance sixteen years ago, and has since gradually increased in size. Its base was composed of conglomerations of adipose tissue, but the middle and apex appeared to be encysted.

It had become complicated in its organization in consequence of repeated attacks of inflammation. It was situated on the head and neck, occupying the whole of one side, extending from the spinous processes of the cervical vertebra to the median line of the neck in front.

It measured twenty-six inches in circumference, and thirty-one inches from its base on one side, over its apex, to the other, and weighed, after it was removed, twelve pounds.

The operation lasted three quarters of an hour. The arteries were ligated as soon as they were divided.—Seven or eight required the ligature, and the strength of the patient was supported by the liberal administration of brandy, given in small doses.

After the tumor was removed, the flaps of the wound were placed in coaptation, and secured in the usual way. A large proportion of it united by the first intention, and in ten days from the time of the operation the patient was able to return home.

I think this tumor is as large as any, if not the largest, which has been successfully extirpated in the United States.

EVANSVILLE, IA., October, 1852.

REVIEWS.

Art. IV.—*Clinical Reports on continued fever, based on an analysis of one hundred and sixty four cases; with remarks on the management of continued fever; the identity of typhus and typhoid fever. relapsing fever, diagnosis, etc. To which is added a memoir on the transportation and diffusion, by contagion, of typhoid fever, as exemplified in the occurrence of the disease at North Boston, Erie county, N. Y.* By Austin Flint. M. D., Professor of the Principles and Practice of Medicine, and of Clinical Medicine, in the University of Buffalo, and Editor of the Buffalo Medical Journal. Buffalo: George H. Derby & Co., 1852.

Dr. Rush was wont to say that a man might as well attempt to portray a summer cloud in a painting, as to attempt to describe a fever. The elements of the disease are undergoing perpetual changes, and Dr Rush looked upon any attempt at seizing and fixing the fleeting movements as an attempt at impossibilities. Those who have devoted their lives to the perusal of treatises on fever, may easily conclude that Claude, of Lorraine, was more successful in fixing the features of summer clouds on canvass, than writers on fever have been in portraying its changing forms. Doctors differ widely in their views, and their differences have given point to a sarcasm. But these differences exist in the varied phases of the human mind, and are not confined to the medical profession. Two artists may view the same landscape, and in their attempts to transfer their impressions to canvass, may

differ so widely that it would be difficult for any one to detect a resemblance. Yet, each may have been honest in his attempts, and faithful in executing his conceptions. Fixed as the features of a landscape may seem to be, a thousand circumstances may change the appearances that strike the eye. That which was dressed in peculiar loveliness, while the first beams of the morning sun bathed themselves in the glistening dew, or lighted up the glossy green of the grass, or enlivened the beauty of the flowers, would, in the roseate blush of twilight, look like a different scene. While the bloom of the Drummond Phlox is in repose, it presents a uniform mass of red; but if the passing zephyr shall curl its beauties, the prismatic colors might envy the varied hues that dance upon the retina of the eye. It is evident, then, that men may differ without mistaking matters. If we wish to do their differences justice, and drink in truth ourselves, we must know how each conception was formed; we must know from what point each viewed the landscape, and the circumstances that governed the view.

If in such a comparatively permanent fixture as a landscape, hese considerations should weigh, how greatly must they be extended when they come into the range of fever, where all is change; where multitudes of circumstances are fixed in nothing but their power to cause variableness. The time may come when all this variety may classify itself, and men may be able to arrange the movements of changeableness into positive groups, as the geologist does his formations. Confusion once reigned supreme in geological science, where now the human intellect has planted beacons that give perpetual light; and if the proper methods of observing and analyzing fever are kept up, we doubt not the path of progress will be well worked.

In the treatise before us, Professor Austin Flint has presented a notable example of patient observation and

industry, of clear and comprehensive views, and of rigid analysis. In undertaking to exhibit his labors, we wish it to be distinctly understood that it must not be taken for granted, that merely because we do not at any point controvert the views or inferences of the author, we necessarily agree with him. Our main object is to point the attention of the reader to the views of Professor Flint, and to show what they are, and occasionally to refer to the foundations on which they rest.

The Clinical Reports originated in the fact that at the annual meeting of the New York State Medical Society, in 1850, the scientific subject which elicited most discussion was the identity or non identity of the forms of fever now commonly known as *Typhus* and *Typhoid;* and a committee was appointed on which the author was placed as chairman, to collect facts relating to Continued Fever, and report at the next annual meeting. The assignment of this duty led the author to examine the cases occuring under his own observation, the histories of which he had recorded; and finding the number considerable, he concluded to subject them to numerical analysis,—and these, for good and sufficient reasons, were published in successive numbers of the Buffalo Medical Journal, and afterwards presented in that shape as a report to the society. In the autumn of 1850, and winter of 1850–'51, another series of cases passed under observation at the *Buffalo Hospital of the Sisters of Charity*, where most of the cases forming the basis of the report just mentioned had been observed. In order to compare the results of the analysis of this second collection with those of the first, the author undertook the labor in the summer of 1851, and published the results in the Medical Journal in which the first series appeared. This second collection consisted of forty-eight cases. As a supplement to these reports, some remarks were contributed on the "Symptoms of Typhoid and Typhus Fever; the identity

of these two types; the diagnosis, etc." A paper on the "management of continued fever," was also added. In the autumn of 1851, and the winter of 1851–'52, another series of cases more numerous than before, passed under the observation of the author. This series constituted the "Third Clinical Report on Continued Fever, based on an analysis of sixty four cases." Appendices were added to the third report. The first is on the morbid condition after death, (exclusive of intestinal lesions,) distinctive of Typhus and Typhoid fever. The facts in this paper are derived from Dr. Jenner's publications in the Edinburg Monthly Journal of Medical Science, in a series of articles commencing with the No. for April 1849, and ending with the No. for April 1850. This series presented an analysis of sixty-six cases of Continued Fever, embracing the two types, Typhoid and Typhus, "the object being to settle the question of the identity or non-identity of the two types by a comparison of the phenomena presented during life, and disclosed with the scalpel after death. The second appendix relates to *Relapsing* fever, and presents the results of an analysis of fifteen cases.

The closing paper of the work is devoted to a "Memoir on the Transportation and Diffusion by Contagion, of Typhoid fever, as exemplified in its occurrence at North Boston, Erie county, New York."

We have thought it due to the author, and to the important subject to give a minute detail of the sources of information that form the basis of the work. No one can doubt that the course pursued by Professor Flint is the correct one for shedding light upon a subject which has many points of obscurity in it, notwithstanding the vast number of tomes that have been written for the special purpose of clearing up all obscurity. The reader of the present work will find himself in the hands of a remarkably intelligent observer, whose teachings deserve and must command respect. The work is in fact a series of Clin-

ical lectures in which an able observer discourses clearly and intelligently upon the phenomena of disease, as they spring to view in the progress of the disease, and they are filled with a fund of instruction that cannot well be taught even at the bed-side.

We do not purpose to enter into an analysis of the statistical details made by Professor Flint. The mere statistics could not be made very profitable to the reader, without an ample reference to the details on which the statistics are founded. Dr. Flint has so perfectly interwoven the various parts of his subjects, that it would be difficult to select any particular part as a sample. But we hope that those who desire to understand all that can be known about the types of continued fever, will not neglect the important contribution made to the subject in this work. It is only by the study of every possible phase of this disease, under the observation of honest, intelligent, and reliable observers, that any one can hope to recognise the ever-varying forms it assumes. These Clinical Reports are very full, and will well reward the diligent student. In order that the reader may see something of the completeness of the plan of observation, we present the elements of the various sections;

Section 1. Age; occupation; civil condition; nativity; habits; season; constitution and previous health of the patient; period of residence in this country, and in this city; duration of the disease before coming under observation.

Sec. 2. The access; Its duration and symptoms; circumstances supposed to have been concerned in the production of the disease.

Sec. 3. Symptoms referable to the general aspect; expression of countenance; decubitus.

Sec. 4. Symptoms referable to the nervous system; mind; sleep; coma; senses and sensibility; muscular contractions, etc.

Sec. 5. Symptoms referable to the digestive system; appetite; thirst; tongue; sordes; parottitis, nausea and vomiting; alvine djections; tympanitis; tenderness of abdomen; gurgling.

Sec. 6. Cutaneous eruptions.

Sec. 7. Symptoms referable to the respiratory apparatus; cough; expectoration; pain in the chest; pneumonitis; aberration of respiratory movements; epistaxis; singultus.

Sec. 8. Symptoms referable to the circulation; pulse; capillary congestion.

Sec. 9. Symptoms (exclusive of eruptions) referable to the skin.

Sec. 10. Symptoms referable to the genito-urinary system.

Sec. 11. Duration of the disease; circumstances attending convalescence; sequelæ; mode of dying; fatality.

Sec. 12. Examinations after death.

Sec. 13. Treatment.

Sec. 14. Cases of doubtful type.

The reader will at once perceive, even from these indicæ, how very comprehensive a scope Dr. Flint has taken in making these Clinical Reports. The objects of investigation, embraced in these subjects, belong to each of the three Reports.

The reader may be desirous of knowing the course of treatment pursued by Professor Flint, and to that we direct attention. The means used are distributed under their appropriate heads.

In *two* cases, both of the *Typhus* type, detailed in the third Clinical Reports, no medicinal remedies were employed. Sanitary measures alone were ordered, and the cases terminated favorably.

Laxatives. These were prescribed in several cases where there had been no dejection for several days. No active cathartic was administered in any case. Half ounce doses of castor oil by the mouth, or in simple

enemas, were the laxative measures. The oil was used in ten cases. Nine of these were of the typhus type. In no case, but one, was the oil given more than once during the course of the disease. In nearly every case it was given early in the disease. A single enema was prescribed in *three* cases, all of the *Typhus* type. In one case where castor oil was given, and in one in which an enema was resorted to, the dejections which followed were so frequent that restraining means were necessary.

Anodynes. These were employed in a few cases to restrain diarrhœa, and the undue operation of laxative remedies. The tinct. opii. was used, taken either by the mouth or by enema. It was resorted to in five cases of *Typhus*, and eight cases of Typhoid. The use was very limited. The author had been accustomed to the prescription of anodynes to relieve mental aberration, and promote sleep. Even where vigilance or delirium was not a prominent symptom, Dr. Flint had supposed an anodyne influence salutary. But in the class of cases embraced in the third clinical report, anodynes were resorted to exclusively for restraining undue activity of the bowels, and the sense of their necessity for any other purpose was lessened by the observation of cases in which they were omitted.

Astringents. Tannic acid was given in only *one* case of *Typhoid*, to restrain diarrhœa.

Sedatives. The articles employed under this head were *tartarized Antimony* and *Potassa*, and *Camphor*. The former was used in *three* cases of *Typhus*, and in one case of *Typhoid*. In the first class it was given in doses of 1–16th of a gr., to relieve delirium. In the *Typhoid* case it was given for pneumonitis. In one case of *Typhus*, the antimony constituted the sole medicinal treatment. *Camphor* was given in three cases of *Typhus* for taxic symptoms, and in no case of *Typhoid*.

Ammonia. The carb. of Ammonia was given when-

ever the circulatory forces were failing. It was used in all the cases ending fatally by Asthenia. It entered into the treatment of six cases of *Typhus*, and four cases of *Typhoid.*

Quinia. This was prescribed in doses of *three* grains, three times daily, and continued several days in *four* cases of Typhus. In each of these cases it was given from the commencement of the career. It was given in one of Typhoid.

Huxam's Tinctnre was prescribed in *one* case.

Vesication. Blisters to the nape of the neck were resorted to whenever spasmodic inspiration was observed, or a tendency to coma. They entered into the treatment of all cases that ended fatally by apnœa. The only exception of their use for other symptoms than those named, was their application behind the ears for external otitis. They were applied to the neck in *nine* cases of *Typhus*, and in four cases of *Typhoid.*

Sinapisms and stimulating liniments. Occasionally employed where pneumonitis was the complication, or where coma existed or was threatened.

Diffusible Stimulus. Brandy was almost the sole agent employed as a diffusible stimulus. It entered into the management of most of the cases of both types. It was omitted in but five cases of *Typhus*, and two of *Typhoid.* In several of the Typhus cases (in all nine) brandy constituted the sole remedial agent employed. The quantity given was generally half an ounce; in some cases an ounce. The intervals of its administration varied from half an hour to four hours. In a large proportion of cases, the remedy was commenced early in the disease, and continued to its close. Prostration, coolness of the surface, feebleness, and, more especially, notable frequency of the pulse were regarded as indicating its free administration. But it was often given to forestall, by sustaining the vital forces, the occurrence of these symptoms.

Nutriment was systematically administered, such as essence of beef and milk porridge. The articles of nourishment were given sometimes against the inclinations of the patients. Cleanliness, ablutions and ventilation, were resorted to.

The only attempts at cutting the disease short, were made with the *wet sheet*, and it was used in two cases of *Typhoid* fever without any useful result.

These various elements of treatment exemplify, therapeutically, the treatment of *Typhus* and *Typhoid* fever mainly by stimulants and nourishment. The rate of mortality was not large.

We present the following synopsis, inDr. Jenner's language, in order that our readers may see what that eminent practitioner has to say on the important subject of morbid appearances distinctive of Typhus and Typhoid fever, based on an analysis of sixty-six cases:

"*Cadaveric rigidity*, ceased much more quickly in subjects dead from typhus than from typhoid fever."

"*Discoloration of the walls of the abdomen and of the skin covering the larger veins*, was much more frequently present in those dead from typhus than typhoid fever."

"*Emaciation* had made greater progress in the typhoid than in the typhus subjects."

"*Spots.* The spots observed during the progress of the cases of typhus fever continued after death; no trace of the spots visible during life could be detected after death from typhoid fever."

"*Head.* After typhoid fever, the pia mater and arachnoid separated from the convolutions with abnormal facility in one only of nine cases examined with reference to this point. The vessels of the pia mater were abnormally filled with blood in one-third of the cases, but intensely congested in one only of the fifteen cases; the cerebral substance was congested in one-seventh of the cases. After typhus fever, the pia mater and arachnoid separated with abnormal facility in nine of eleven cases of which notes on the point were made. The vessels of the pia mater were congested in nearly half, and intensely con-

gested in one-fifth of the whole of the cases; while the cerebral substance itself was abnormally congested in half."

"*Hemorrhage into the cavity of the arachnoid*, which was not found in a single case of typhoid fever, had occurred before death in one-eighth of the cases of typhus fever." "The amount of serosity found within the cranial cavity was decidedly greater after typhus than typhoid fever."

"*Pharynx.* After typhoid fever, this organ was found ulcerated in one-third of the cases. After typhus fever, ulceration of the pharynx was not detected in a single case."

"*Larynx.* Ulceration of the larynx was found in one of fifteen subjects dead from typhoid fever; in one of twenty-six from typhus fever."

"*Œsophagus.* After typhoid fever, ulcerated in one of fifteen cases in which it was examined. After typhus fever, the œsophagus was free from ulceration in all the twenty-four cases in which it was examined."

"The epithelium separated from the œsophagus spontaneously at an earlier period after death from the latter than the former disease."

"*Stomach.* In none of the fifteen cases examined after death from typhoid fever was the mucous membrane of the stomach softened throughout the whole extent; in no case did softening of the cardial extremity approach perforation. In four of thirty-seven cases of typhus fever the whole mucous membrane of the stomach was softened; and in four others there was such extreme softening of the whole of the coats of the great *cul de sac*, that they were perforated by the slightest violence.

"*Small Intestines and Mesenteric Glands.* The presence or absence of lesion of these organs was the ground on which the cases of typhoid and typhus fever here analyzed were divided from each other—consequently they were invariably diseased in the one, and sound in the other."

"*Large Intestines.* After death from typhoid fever the mucous membrane of the large intestines was found ulcerated in rather more than a third of twenty cases In no instance after death from typhus fever.

"*Peritoneum.* As peritonitis was in typhoid fever secondary to, and dependent on the entero mesenteric disease, it may here be excluded from consideration."

"*Spleen.* This organ was enlarged in all the cases of typhoid fever—softened in one-third of the cases only. Before the age of fifty, it was as large after typhus as typhoid fever; after that age, it was decidedly smaller in the former than in the latter affection. After the age of fifty, it was as soft in typhus as in typhoid fever; before that age it was frequently softened."

"*Gall Bladder.* There was ulceration of the lining membrane of the gall bladder in one of fourteen cases of typhoid fever; in none of thirty-one cases of typhus fever. In the latter disease the bile was much thicker, and of a darker green color than in the former."

"*Liver, Pacreas, Kidneys.* These organs were more flabby in the cases of typhus than in those of typhoid fever."

"*Ordinary Bladder.* This viscus was ulcerated in one of the cases of typhoid fever—in none of the cases of typhus fever."

"*Pericardium.* This cavity contained a small amount of yellowish transparent serosity in all the cases of typhoid fever examined. The contained serosity was red, from transudation of a solution of hæmatosin, in five of thirty-one cases of typhus fever, in which the pericardium was examined before the termination of the fever."*

"*Heart.* The muscular tissue of this organ was much more frequently and decidedly flabby, and its liniag membrane was much more frequently and deeply stained of a dark red color, in the cases of typhus fever, than in those of typhoid fever."

"*Lungs.* Granular and non-granular lobular consolidation were very frequent in the subjects dead from typhoid fever—rare in those dead from typhus fever. The reverse was the fact with reference to congestion of the most depending part of the lung."

"*Pleura.* Recent lymph or turbid serosity was found in six of fifteen cases of typhoid fever—i. e., between half and one-third, or in the proportion of forty per cent.

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* There is evidently a mistake in the last sentence.

The same lesions not much less in amount were found in two only of thirty-six cases of typhus fever—i. e., one-sixteenth, or in the proportion of fifty-five per cent."

The foregoing is copied from the Monthly Journal of Medical Science for April, 1850.

Dr. J. adds, after the foregoing parallel of pathological appearances, as follows: "The particulars here briefly recapitulated, and still more those fully detailed in the foregoing papers, appear to me to prove indisputably that the symptoms, course, duration, anatomico-pathological lesions, and the tendency to cadaveric changes, are different in typhoid fever from what they are in typhus fever."

On the subject of relapsing fever we should like to present a full analysis of the views of Professor Flint, but we must content ourselves with such a notice of this form of fever as will enable our readers to observe and detect it. We have seen several cases of this apparent mongrel variety of fever, and its phenomena are at times very embarrassing. The following is the synopsis made by Professor Flint:

Evidences of a form of fever supposed to be identical with *Relapsing fever*, have heretofore received different names, such as the "short fever;" the "five day fever;" the "seven day fever;" "bilious remittent fever;" "remittent icteric fever;" "mild yellow fever," etc.

The disease is stated to affect all ages, and both sexes in about an equal ratio.

The *access* does not present any very distinctive features. The attack is oftener abrupt than in typhoid fever, and the muscular and articular pains are apt to be severe.

Delirium, and other head symptoms are represented to be oftener absent, and, when present, less in degree than in the other forms of continued fever.

The absence of the abdominal symptoms which are so generally present, in a greater or less degree, in *typhoid* fever, is an important point distinguishing *relapsing fever.* Diarrhœa, with notable meteorism, and tenderness in the

iliac regions, do not belong to the latter. It is, however, characterized by certain symptoms pertaining to the digestive system rarely found in *typhoid*, viz: nausea and vomiting, which are frequently prominent symptoms, and tenderness over the epigastrium. The matter vomited is stated to be "bright grass green," and sometimes like coffee grounds, approximating to the black vomit of yellow fever.

The eruptions characteristic of *typhus* and *typhoid*, are not found in *relapsing* fever. Sudamina and occasionally petechiæ have been observed. An eruption consisting of "small spots, round, purple, unaltered by pressure," has been described by some observers as an element of the affection. These spots are stated to be extremely like flea bites, and it does not appear to be fully ascertained that they were not, in fact, flea bites. The chest symptoms, i. e.: cough and bronchial rales, so generally present in *typhoid* fever, are said to be less commonly present in this disease. *Epistaxis*, so frequently observed in *typhoid* fever, sometimes occurs. The pulse, seldom falling below 100, in more than half the cases is 120, and in a considerable number the frequency is still greater.

Profuse sweating is stated to occur pretty uniformly, preceding the apparent convalecence and also when the relapse of fever is about to terminate. Another distinguishing feature referable to the skin, is more or less yellowness, occuring on the fourth or fifth day. In severe cases the jaundice is often a prominent symptom. The most distinctive event, however, is that indicated by the title of the disease, to wit: the occurrence of relapses. The first febrile career continues for a period rarely less than *four*, nor more than *ten* days, and then ends, generally after profuse sweating, which is considered to be critical, leaving the patient free from febrile movement, and apparently convalescent. After an interval varying, in duration from *five* to *eight* days, another attack occurs, generally abruptly, and frequently preceded by a chill.—The recurring febrile movement is equally or more severe than the first. It continues from four to five days, and then terminates usually after profuse sweating. Usually after a single relapse, the patient becomes permanently convalescent. A second, third, and even a greater number of relapses, however, have been observed to occur.

The disease is seldom fatal. Patients almost uniformly recover, unless some complication takes place, such as pleurisy, pneumonitis or dysentery.

The most distinctive characters derived from autopsical examinations, are the absence of the intestinal lesions belonging to *typhoid* fever. The peyerian glands are unaffected. The spleen is generally enlarged and softened.

It may be communicated by contagion, and Dr. Jenner, in the article already referred to, has collected observations tending to show that relapsing fever cannot be derived from patients laboring under the other forms of continued fever, but that it alone produces the special miasm for its *propagation*. How far the facts contributed by him go to establish this important point, it does not fall within my present purpose to inquire.

This form of fever does not secure immunity from the other forms; nor from subsequent attacks of the same form.

These are, briefly, the more prominent of the traits distinctive of *relapsing* fever which are mentioned by the two writers to whom I am indebted for the foregoing summary. They appear to have been deduced from an examination of reports of epidemics, supposed to be of this character, by various authors. If they are sufficient to render probable the position that the disease to which they relate is a peculiar form of fever, distinct from the *typhus* and *typhoid* forms, it is certainly desirable that collections of cases should be subjected to careful numerical analysis. The question can only be definitely settled by the results of such analysis.

So far as I know, we have no account of a form of fever prevailing in this country, analogous to the relapsing fever as just described. Were the cases characterized by relapse, among those observed by me during the winter of 1850–51, cases of *relapsing* fever? With a view to arrive at the answer to this question, and, if the conclusion be in the affirmative, to furnish a small contribution toward the natural history of that form of fever, I will now proceed to analyze the cases referred to.

We must here close our analysis of this valuable contribution to the subject of Fever, and pass to the consid-

eration of Professor Flint's paper on "The Transportation and Diffusion by Contagion of Typhoid Fever." There is nothing about Typhoid fever that has called forth a greater variety of opinions than the means of its transportation and diffusion. How far the epidemic character of Typhoid fever may sustain the doctrine of contagion, we are not prepared to say. We must frankly say that we have never seen any thing, under our own observation, that would justify a belief in contagion. We think the disease is somewhat rare in Louisville, because we have seen but little of it here in the course of many years. Very recently we saw four cases in one family. In this instance we know of nothing that would lead any one to suspect contagion. There were no cases from whence these subjects might have caught the disease, and numerous children visited their sick friends, but no one of them received any infection. There were young members of the family exposed to whatever there may be of infection in the disease, but they did not suffer from the exposure. These are facts that we observed, but we do not urge them as militating against the doctrine of contagion; we mention them only as facts observed. They are too limited either to establish or destroy a doctrine.

It is evident, however, that the belief in the contagiousness of Typhoid fever is growing, and we are assured that there are observers, who commenced making memoranda, as anti-contagionists, who are surprised to find facts accumulating among their records, that strongly point to contagion. While we remain unconvinced, we acknowledge that there are appearances in favor of the contagiousness of the disease, that cannot well be accounted for or explained if we deny the doctrine of contagion. The paper of Professor Flint, of which we are about to make a synopsis, has already converted former skeptics, and fixed the faith of some who were wavering.

It is due to the subject that the facts drawn from the fever of North Boston, Erie county, N. Y., be fairly presented.

North Boston is about eighteen miles from Buffalo, and twelve miles from the shore of Lake Erie. It was a salubrious spot. No malarious source was known in its neighborhood. Neither Intermittent fever, nor any of its kindred types, had prevailed for several years. Typhoid fever had never appeared in any part of Erie county. Remittent fevers had been the usual types, but these have diminished, and Typhoid fever has proportionally increased.

In 1843, the year of the fever, there were but nine families in North Boston, all living within an area of an hundred rods in diameter; but the few houses in which the disease occurred were closely grouped together, the house farthest removed from the tavern being only ten rods distant. Forty-three persons made up the entire community.

In September, 1843, a young man from Massachusetts being on a journey westward, took lodgings at Fuller's tavern. He had been ill for several days, and stopped at North Boston because he was unable to proceed any farther. He remained at the tavern, and died on the 19th of October. The symptoms establish the fact that it was a case of *typhoid* fever. In the portion of Massachusetts from which the stranger came that is the prevailing form of fever.

Between October 19th and December 7th, twenty-eight of the forty-three inhabitants of North Boston were attacked with fever, and ten of the cases proved fatal. The first case was in a son, aged sixteen, of the inn-keeper. He was attacked twenty-three days after the arrival of the stranger. About the same time a daughter of the inn-keeper was taken ill with the fever; then another daughter, about twelve days later, and another aged three years, about the same time. Another son of the inn-keeper was attacked on the 9th October. Two othe

cases occurred in this family. Seven cases, exclusive of the stranger, occurred in this family.

A son of the inn-keeper, a married son, living about four rods from the tavern, who visited his father's in order to assist in nursing, was attacked on the 15th of October.

About eight rods from the tavern a family of the name of Penhallow was attacked. In the family of Ingraham, residing about three rods from the tavern, were seven cases and three deaths. A family named Hallick resided about ten rods from Fuller's, and seven cases occurred in this family, of which five were fatal. A family named Dobbs, about three rods from the tavern, had four cases, all of whom recovered. Another case occurred in a family living within twenty feet of the tavern.

Only three families in the settlement escaped. One of these lived in a part of the house occupied by the inn-keeper's married son. This was the only family in the restricted area of the disease that escaped. Another family, named Sprague, lived about forty rods from the tavern. Bailey's family also lived about forty rods from the tavern.

In the families that were attacked there were individuals who escaped. Their names and ages are as follows: Fuller, the inn-keeper, about 50; Ingraham, about 50; Dobbs, about 45; Penhallow, about 40, and Hallick, about 40. Fuller was in comfortable circumstances. The other families were poor, almost to destitution. The families who escaped were in good circumstances.

In the mysterious circumstances attendant upon this crushing and new calamity, the imagination rioted.—Wiseacres, as usual, settled, to their own satisfaction, a problem that puzzled philosophers. Untrammelled by those absurd rules that logicians call laws, these popinjays jumped to conclusions without undergoing the tediousness of a journey of ratiocination. Their dictum was sufficient, and all were fools who did not leap over

such things as facts, and seize their doctrine without the trouble of reasoning. These jumping sages found the cause of this disease of New Boston in a poisoned well, just as their similitudes in the dark pestilences of Europe always found the cause of epidemics in water-fountains. They probably had heard that truth lies in the bottom of a well, and for either that reason, or probably for not as good a reason, located the origin of pestilences in wells and springs.

In New Boston the family of Fuller, where the disease commenced, and the family of Stearns, who escaped an attack, were engaged in a bitter feud, and as the disease was ascribed to poison, administered by Stearns, all the families of the settlement cut the acquaintance of his family. A well, near the tavern, was used by all the families except that of Stearns and one other family. It was charged that Stearns had poisoned the well, and futile efforts were made to empty it with pumps. Up to a short period before the outbreak of the disease, the family of Steanes had been supplied with water from Fuller's well, but, owing to the feud referred to, the family of the former had been cut off from the well, and had been forced to make a well for themselves. Another of the three families that escaped did not use the water of Fuller's well. By the others it was used daily. Upon these coincidences the assumption was formed that Stearns had poisoned the well. Some of the water was taken to Buffalo and analysed. The water was found to be remarkably pure. The excitement against Stearns was of course very great, and he instituted a prosecution for slander, which was settled by the party prosecuted by the payment of one hundred dollars.

The mysterious character of the disease, its unusual phenomena, and the poverty of the sufferers, called for medical aid from Buffalo, and Dr. Flint visited North Boston. He satisfied himself by extended observations,

and patient examination, that the disease was Typhoid fever. Autopsies revealed the characteristic lesions of Typhoid fever.

Dr. Flint supposes that the miasm of contagion is much more abundant, or potent, at certain times and places, than when the disease ordinarily prevails. We know that this is the case with the miasm of small pox, for that seems to be almost governed in its virulence by determinate periods. "Or," says Dr. Flint, "it may be contended that the miasm, which generally is inoperative, becomes more efficient; not by any increase of its strength or quantity, but by the co-operation of other agencies, which favor its operation, or render individuals more susceptible to its influence."

We admit that the facts bear strongly towards the doctrine of contagion, but there are facts, even in this strong array, that seem to tell against it. We know of no contagion that pays respect to age; and a number of persons, of forty years and upwards, escaped an attack, though they were in the very focus of exposure. It strikes us that this is an obstacle to the fullness of the proof of contagion at North Boston. The facts and arguments of Professor Flint are stated with clearness, force and entire honesty.

We cannot quit this book without giving our cordial thanks to Professor Flint for his valuable contribution to the study of fever. He has acquitted himself with the highest degree of credit, and we commend his work as one of the best monographs that has appeared on the subject.

ART. VI.—*Report of the Medical Dipartment of the University of Pennsylvania, for the session of* 1851–2 *to the alumni of the school.* By the MEDICAL FACULTY.

There are some curious features in the document, the tiile of which we have given above, that naturally challenge the attention of the medical profession. The American Medical Association has suggested, as a measure of reform in medical teaching, the prolongation of the lecture terms of the schools to six months, and the medical department of the University of Pennsylvania leaped into the measure with a celerity that bordered on bustling. Year after year she made the air sonorous with her bugle notes on the subject, and from her importunities to the American Medical Association, she seemed to have taken up the idea that the sole object of the existence of that body was to hold up the hands of the good old Moses of medical reform, in the battles with the Ammonites. She constantly occupied the annual meetings of the Association with her bills of charges for the great sacrifices she had made in furthering the suggestions of the Association for reform in teaching.

But a change has come over the sturdy champion of long terms. The circular recently issued by the faculty, of what is known as the old school of Philadelphia, thus announces her position in the arena of reform and competition

"It is well known to the friends of this school that it has been engaged in an earnest effort to improve the system of medical education in the United States. One of the means by which it has sought to accomplish this object has been the prolongation of the scholastic term of instruction."

The reader must remember now that in our quotation the object and motive which led to the prolongation of the lecture term are set forth, and that the claim is urged that the object was exclusively the improvement of the

system of medical education in the United States. It is very important that this shall be remembered, for, if forgotten, the reader cannot appreciate the curious attitude that "circumstances," as potent sometimes in medical as in political circles, have forced the University of Pennsylvania to take.

In 1849–50 the term of that school had been extended to six months. By some this prolongation was looked upon as a vigorous stride towards improvement. Others took a reverse view, and we were among the number.—The Harvard school opposed the extension, and based her opposition on ample reasons.

But the school of the University of Pennsylvania claimed that she made the prolongation in obedience to an earnest wish to improve the system of medical instruction in the United States. She annually filled the ears of the profession with testimonials of the gratification she experienced from this great reform. Her evidence to its vast merits was unqualified. But, at the same time, her faculty announced that they were quite aware of the risk incurred; but as no improvement could be expected unless some school would venture to take the first step, "ours, as the oldest, and among the most prominent, seemed especially called on to set the example." Thus it was that a sense of duty pushed the school towards the fires of martyrdom, and as the first effects were found to be "excellent," in the estimation of the faculty, that body urged the general adoption of the prolongation with a vehemence quite worthy of the holy cause. Even in the present document the faculty remark that they are gratified in being able to say, as they do with perfect sincerity, that "there has been a progressive improvement in all respects, increasing steadiness of deportment, attention to study, and earnestness of self-improvement have been observable; and the examinations, though not less strict than formerly, have each year resulted in

relatively fewer failures to attain the honors of the Institution." Here then we have a most satisfactory result of reform; the object of the reform being the improvement of Medical Pupils, seems to have been fully realised by the University of Pennsylvania. The Faculty had felt that their Institution was a city set on a hill, whose example was essential, and whose light should not be hid. They felt that they were adventuring, but were encouraged by duty; and according to the Faculty, the great reform answered *every end* that had been promised for it, by those who suggested it. Of course the reader is prepared to hear that the old and prominent Institution is persevering in the good works that brought forth a plentiful crop of excellent fruit. But, alas! although the classes improved in their steadiness, excellence, earnestness in study, and in capacity for standing examinations for the honors of the school; although everything worked well, there was a debenture; the classes began to fall off. The reform had worked admirably in qualifying students for the doctorate, and for practice, but the Professors found that their fees were diminishing, and after all the "earnest efforts to improve the system of Medical education in the United States," after proclaiming year by year that the "earnest efforts" were perfectly successful, in the very midst of these elevating and glowing prospects, the Faculty announce that they cannot stand the benefits of the reform any longer. The reform was a good thing to write about, to talk about, to trumpet over the earth, to bring annually before the American Association; it accomplished everything that had been promised for it, it improved students most essentially, made better Doctors, thereby offering something to society, for low per centage in some of the columns of vital statistics, but, notwithstanding all these things, the Faculty had discovered that it diminished, not a fair and reasonable compensation, but to a small extent an extravagant income, if compared with the revenue of a large majority of the Medical Schools of the United States,

and on account of this diminution, the Faculty turn their backs upon that reform, and its good results, which they had lauded annually.

After all the "encouraging results" that have been ardently and perseveringly pressed upon the profession for years, as the fruits of six months terms of lectures; the reader is scarcely prepared to hear that the Faculty have determined to shorten the "good time" to a five months term! The Faculty do not pretend that they have changed their views of the excellence of what they deem the essential results of the long term. But what? Why, the fees are diminishing. At first the classes increased, and then the reform was magnificent; now the classes are lessening, and though the reform was, and is, according to the circular, beneficial to the medical students, to the profession, and to society; yet the Faculty are done with it. The first draught made at extension, caught 509 matriculants, the second 499, third 439, fourth 466, and for 1851–'52, only 410. The class of 1851–'52 opened the eyes of the Faculty to the enormity of reform. The age and prominence of the school induced the display of a love of reform, but "prominence" and long columns of matriculants being matters of some moment to the school; as soon as shadows seem to fall on them, the reform is hurried overboard with hot haste.—What if students were improved, what if the graduates under the long term were better qualified to practice medicine than the graduates under the former system; what if society had its chances of living increased, was the income of the Professors nothing? Could they be expected to benefit the profession and the community on a class of four hundred and ten matriculants? The short class shortened the term, and whether the intention is to continue to shorten the term, if the classes keep up the unpropitious feature of growing less by degrees, we are not informed. It was hoped, by the Faculty, that they would force other schools to follow in their wake, and

that the classes would increase; in both these there was a sad mistake, and the cry is, now, perish reform and all its works.

If it were not for their connection with medical dignity and honor, the efforts of the Faculty to relieve themselves from, what they evidently consider a natural suspicion, that their devotions to Medical Science, and "earnest efforts to improve the system of medical education," are more influenced by the effect of the "efforts" on their income, than by the good results of reform, would be infinitely amusing. The "efforts" were "earnest," indeed, as long as the class increased, and the Faculty admit now, that the results to students, and to society were satisfactory. It is a plain matter of fact, that the Faculty did not feel inclined to submit to any sacrifice, in advancing the interests of the profession. The interests of the Profession and of the Professors, must run *pari passu.*

We do not object to the changes in the system of managing the affairs of the Medical department of the University of Pennsylvania. What we object to is, to see the Faculty strutting through the land with very broad phylacteries, covered with texts of scripture of exceeding devoutness. All men cannot, at will assume airs of disinterestedness for an emergency, and we do not like to see those who can, too lavish of the display. If the circular of the Faculty had fairly stated the fact, that the University was favorable to Medical reform as long as that reform swelled the income of the Professors, and that in spite of the admitted great benefits of the reform, to the profession, and to society, the Faculty deserted it, when their classes began to diminish in number; no one could have objected to the exercise of the right of the Faculty to make such a public announcement. It is true, there might have been cavillers who would have said that a class of four hundred students should be sufficient to gratify any pride of prominence, and any reasonable desires of income. Some might have gone farther, and ex-

pressed the belief that a class of that size should have been sufficient to keep a Medical Faculty from deserting a good cause that was working well. The "prominent school" of the country was scarcely justified in withdrawing from a fair test of a new educational scheme, for no better reason, than because matriculants were diminishing in numbers to some extent. It is a sorry lesson to Medical schools, whose best emoluments are far below the liberal scale that frightened the Faculty of the University of Pennsylvania. These schools find in the indisposition of the Faculty of that Institution, to jeopard in the least degree their emoluments in favor of a reform that they said worked admirably, an excuse for not making any "earnest efforts" to improve themselves, their pupils, or the community. Alas! for science and philanthropy.

The evidence of this position of the Faculty of the University of Pennsylvania, given by their own circular is as clear as the noon-day sun. We cannot believe that they hold the intelligence of Medical readers in as low an estimate as they indicate, in presuming that educated minds were to be deceived by the specious ethics in which the retreat from the good work of reform is attempted to be concealed. The sleeping draught was made so strong, that the narcotic is easily smelled. But the reader may incredulously ask, can these things be? Are you not giving us distorted inferences, instead of facts? Let the doubter read the following extracts from the circular:

"There are two distinct principles upon which a medical school may be conducted; one having reference to the interests of the institution, the medical profession, and the country; the other to the temporary and pecuniary interests of the professors.

If the first of these principles be acted on, the efforts of the school will be so directed as to secure the highest possible qualifications in the graduates. Not only will the amplest opportunities for instruction be afforded, but rules will be established, and, as far as practicable, carried into effect, for securing to the pupil the full advantage of

these opportunities. Certain prerequisites to graduation will be announced, upon the basis of which the public may estimate the value of the degree; and, in order that the public may not be deceived, these prerequisites will be religiously adhered to, unless in a few exceptional cases, in which their strict enforcement might work great individual hardship.

The operations of a school based upon the second principle referred to, that, namely, which regards the temporary and pecuniary interests of the professors as the main object, it is unnecessary to follow out in their details.—Of course, they will be directed to the securing of the greatest practicable amount of income; and all measures will be deemed legitimate which are admissible in the struggles of mercantile competition.

The Faculty of the University need not say that they acknowledge fealty to the first of these principles. Independently of the obligations of common honor and honesty, they consider themselves the depositaries of a great trust, in the proper management of which their personal credit is involved. The school with which they are charged has come to them with a reputation, based upon a long course of honorable and successful effort on the part of their predecessors. They consider it a duty to maintain this reputation by all the means within their reach, even at the risk of loss to themselves individually. They dare not compromise the character of the school by lowering its requisitions and relaxing its rules, though by doing so they might possibly for a time increase the number of matriculants, and of course their pecuniary income. They have felt themselves bound, so far as might be in their power, to keep the business of medical instruction in equal advance with the other great national interests. This could be done only by taking an occasional forward step, so as to test the capabilities of the country. It does not follow that such an experiment must always be successful. Should it on trial prove to have been premature, no pride of consistency should prevent a retracing of the step, until it is ascertained at what point a firm stand may be taken, in accordance with the best interests of the school and the profession. The size of the classes will always enter somewhat into the estimate of the standing of a school; and a certain number of pupils is, for

obvious reasons, essential to its efficient operation, under a system by which such institutions support themselves. While, therefore, aiming at improving the character of the classes, the Faculty must not be indifferent to their numbers. As before stated, they are happy in the conviction that, in the former of these respects, there has been for several years a gradual improvement under the improved arrangements of the school. But it must also be admitted that, under the same arrangements, with the opposing influence of a powerful competition, the numbers have not been of late altogether satisfactorily maintained; and there is some reason to apprehend that, without change, there may be a still further decrease."

That language is certainly plain enough. We feel as though Horace was thinking of something of the kind when he wrote:

I know the right, and I approve it too;
I hate the wrong, and yet the wrong pursue,"

Amidst all the flaunting of the "considerations by which the Faculty were influenced;" is anything more conspicuous in bringing about "certain modifications of the recent course," than that which relates to the "temporary and pecuniary interests of the Professors." While we read the flaming zeal for "the first of the principles," for the management of Medical Schools, we feel that here is the proper spirit. We enjoy the sly thrusts made at those "operations of a school based" on that "which regards the temporary and pecuniary interest of the Professors, as the main object," and all seemed so overflowing with high toned Medical honor and zeal for the interest alike, of the profession and of humanity, that we were about to throw the rest of the circular aside under the conviction that all was right, and rejoice over the glory of the Faculty thus pure and disinterested, when our eyes fell on th following: "a certain number of pupils is, for obvious reasons, essential to *its* [the school] efficient operation under a system by which such Institutions support them-

selves." A "certain number of pupils" has not a very remote bearing on "the temporary and pecuniary interests of the Professors." We began to think that two chancery clerks had been dining together, and mixed a chancery bill and cross bill together, to the utter confusion of equity. Are five hundred pupils any better for the trial of an improved system of medical education, than four hundred? And is it exactly honest to give a class of five hundred all the vast advantages that were claimed for the long session, and withhold these benefits from the class of four hundred, merely because five is something more than four? May not the people, instead of being contented to know that a physician is a graduate of tne University of Pennsylvania, find it to their interest to inquire whether at the time of his graduation, the class consisted of five, or four hundred, and graduate their confidence on the numerals, instead of the Diploma?

But, why insult public intelligence by pretending that the Faculty of the Medical Department of the University of Pennsylvania, in their abandonment of the long term, were not influenced by private interests? How else is the "character of the school to be compromised," by a reduction of its class to four hundred or less, while the amount of instruction, and requisites for graduation are increased, and while the smaller classes have exhibited, as the Faculty say is the fact, a decided superiority in diligence and attainments over the preceeding larger classes? If the reform was essential, and demanded sacrifices on the part of Professors; had not the Medical public a right to look *up* to the University of Pennsylvania for a noble example? Who would have expected to find her in such a cause, by looking *down?* Who would have supposed that an Institution of such lofty claims to virtue, could have been turned aside from an "earnest effort to improve the system of Medical education in the United States," by the jingling of monies? Are we to understand that in

all questions for improving Medical teaching, and elevating the profession, the conduct of the Medical Department of the University of Pennsylvania is based entirely on a view of the profit and loss account of the Professors? Are we to understand that on all such subjects, what ever amount of "fine sentiments," which, Joseph Surface like, she may beat up into flummery, it is merely to be poured over bags of dollars, to conceal the true state of case from the eyes of a generous, liberal and philanthropic profession, whose members must be hood-winked for the benefit of Diana of the Ephesians?

The sophistry of the circular, to which we have been referring, is too transparent to deceive many beyond the range of its writers. They possibly worked themselves up to the hope that the tangled web might catch the unwary. Why not make the "modification" without any explanations? Why not make it and repose in grim dignity? Why rouse the public mind by raising the question of disinterestedness? Why dress in the costume of Joseph Surface, and claim to be sailing with John Howard, in the circumnavigation of Humanity?

It seems to us that this is one of those instances of self deception, of which poor human nature has given many examples. The surface of the circular seems to show a deliberate attempt at deception, but we think the Faculty were really hood-winked themselves. This is the charitable construction, and that we prefer. When a man is sensible that appearances are against him, in matters affecting personal motives and actions involving self interest, he cannot be too guarded in giving reasons for his conduct. The liability to self deception is great. While the interested party labors earnestly for excuses and apologies, and rolls forth in majestic streams, swelled beyond their nature, phrases about benevolence, disinterestedness, and philanthropy; a cool, dispassionate observer thinks that the truth would be best expressed by such

terms as avarice, self-interest and cupidity. The veiled prophet of Khorassan was not the last of his race who concealed the most hideous features behind a lustrous veil. Joseph Surface occasionally rises from his grave, and stalks the earth in high-heeled boots, without preliminary "rappings."

The reader may think that the curiosities of the circular are exhausted, but this would be a great mistake. A snarling critic would find a feast in the crumbs that are left, but we shall not indulge our palate. Let us do justice, however, to truth.

While the Faculty of the Medical Department of the University of Pennsylvania were vehemently pressing the superlative advantages of the long term upon the attention of the profession; while they were almost over zealous on the subject, before the American Medical Association, there were many intelligent persons who denied the claim of the University of Pennsylvania to the use of the long term. It was often called a specimen of hocus-pocus. We well remember being present when Professor Miller, of Louisville, delivered a speech before the American Medical Association and stated a series of invulnerable facts that proved the hollowness of the claims to the use of the long term. There was a good deal of fillibustering in an effort to answer Prof. Miller, but his masterly facts were incontrovertable, and the *long term* of the Medical Department of the University of Pennsylvania, received a blow then, from which it never recovered. But at that time the entire existence of Medical education seemed to be staked by the University of Pennsylvania, on the six months term. There was no salvation then, except under six months training, and the "old Philadelphia school" was regularly in the field as the champion of the six months avatar.

We have now two pictures here, and let the reader look first on this picture, then on that. If he will bear in mind

how persistently the Faculty of the Medical Department of the University of Pennsylvania, pressed upon the profession the vast advantages of the *six* months course; how, year after year the most wringing appeals were made to the American Medical Association, to push other schools into the measure, urging that the experience of the Faculty had shown that the essential gains of the *six* months term were increased excellence in the deportment of the pupils, enlarged Medical culture, and a greater ability to pass examinations for Medical honors; and if he has met with many of the numerous discoverers of perpetual motion, and remembers how they glowed over their achievements, such, a reader may enjoy the portrait which the Faculty drew of the greatness, vitality, and essentiality of a *six* months term. That was *the* discovery of the age—the *ne plus ultra* of Medical reform; one for which "prominence" and "old age" could afford to make "earnest efforts in improving the system of Medical education." But, alas! Catalogues began to shrivel a little, and then came "modifications" of the "earnest efforts."

Now, let us look upon the portrait of the "modifications;" a portrait from the same easel, from which the other painting came. The same Faculty that lauded the six months term, and the effects, are now agonising the Medical mind about a *five* months term, and they declare that their Institution neverreally had a six months term, and for fear that some of their credulous friends, who had been convinced of the actual of a six months course might doubt the new phase of affairs, the Faculty enter into an elaborate proof to establish the fact that the Medical Department of the University of Pennsylvania never had such a term. The Faculty *now* insinuate that one week was *wasted* upon introductory lectures; one week of *eight* days was a holiday, a cessation from all labor, and that two weeks, at the last of the session, did

but little service, on account of the want of intercommunion between the Professors, and their restless, worn-out pupils. The proof is decided, and the grand result of the "earnest effort" is, that at least one *sixth* of the long term was devoted to efforts that were neither useful nor ornamental!

The mythology of the Latins taught that Saturn devoured his offspring, but even that wild medley of prurient fancies, did not represent the god as first lauding the altitude and magnitude of his progeny, and then depreciating them before sitting down to his repast. The University of Pennsylvania does not seem to have as voracious an appetite as that of Saturn; she eats rather in the mood that adorned the repast on leeks, which Ancient Pistol accepted at the hands of Fluellen, using the very grace of Pistol:

"I eat, and eat, I swear."

But let us return to the Circular. It says:

"The "Modifications" adopted, will "obviate in some measure, one of the main difficulties on the part of the pupil," without *in any degree derogating from the *great object of improved instruction!*" Indeed, the Faculty go on to present, in a pretty strong light, the advantages of a five months term. And now it appears that there were certain drawbacks to the efficiency of the six months term. "One of these," to quote the language of the report, "is the impossibility of retaining all the students of the first course till the end of the session. Even when the term is short, there is a strong disposition on their part to anticipate the close of the lectures, in their anxiety to return to their home and friends. This disposition is, of course, greater in the prolonged session, and becomes almost irresistable when the example of an early dispersion among the pupils of other schools, in the same neighborhood is placed before them. Young men who see their acquaintances returning home on the first of March, must be gifted with extraordinary self-control, to be willing to linger on till the first of April. *Hence, by*

*The Italics are ours,

*far the larger portion of students not on the list of candidates, and residing at a distance, leave the school as much as two weeks before the *close of the six months session.*"

The report proceeds to mention another objection to the six months term, which is peculiarly applicable to Institutions in which the annual classes are large. We quote as follows:

"Another circumstance qualifying the advantages of the length of terms, has been the necessity of examining the candidates for graduation during the continuance of the lectures, in order that they might not be detained long after the close. But with the prospect of the examinations before them, to the result of which they look with the greatest anxiety, as being likely to influence their whole future life, the young men find it impossible to command a due attention to other objects; and all those objects are consequently neglected, which have no direct bearing on the approaching time. *By the candidates, therefore, as well as by the pupils of the first course, the lectures of the last two weeks of the session are but partially attended.*"

The report adds:

"It has long been the custom, even before the lengthening of the session, to grant the class a recess from Christmas to New Year's inclusive; and the first week of the course has generally been occupied exclusively with the introductory lectures. *Thus two weeks have *been nearly lost to the session.* This has been the case, not only with the University, but also, with other †schools having shorter courses."

The Faculty say, that "from the above remarks it is evident that, though the session in our school has been prolonged to six months, and has fully occupied this period according to the system pursued before the change was made, so that the six months term was really in advance of the former term to the full nominal amount, yet such were the arrangements that a length of time equivalent

*The various Italics are ours.

†This statement may apply to Philadelphia schools, but not to all those in other places.

to about four weeks, if not lost to the business of tuition, was certainly not occupied in the most effective manner."

"Now, by modifying these arrangements, the Faculty hope to be able to accomplish all, in the way of instruction that has been gained by the prolongation of the session, while they spare to the first course student three weeks, and to the candidate one week of the time, and an equivalent proportion of the expense which the six months system cost them."

The Faculty had so repeatedly assured the public that they were "earnest" in six months "efforts," that they no doubt felt that all this elaborate argument was absolutely necessary to exorcise the spirit they had been conjuring for years. Even now, we fear, that in spite of the ample confessions of the Faculty that the six months course was regular humbugging, there are many who were won to the former faith by the asseverations of the Faculty, who may cling to their old faith. Indeed, we think that none could be fairly charged with the commission of an outrage, who might charge that the Faculty of the "Old Pennsylvania school" did not write the circular. They might fortify themselves by asserting that there is not a fact, nor an objection as to the lapses of the *long term*, that was not known to the Faculty, when year by year they were proclaiming that they were honestly engaged in the "earnest effort" of the full six months term. The sturdy doubter might add, that surely that Faculty would not stun the senses of the profession by repeatedly asserting the immense results of six months teaching, and now proclaim that their students never really had more than a five months course! Perhaps some one might add, that if the present statements of the circular are true, some awful monstrosities have been palmed upon the profession, from the same quarter. Does that Faculty hope that the profession has taken chloroform, in order that these unconscionable things may be slipped in during a state of unconsciousness?

The "modifications" referred to, consist in giving the introductories in two or three days instead of appropriating a week to this purpose; limiting the recess or holli-days to Christmas and New Year's day; convening the

session on the second, instead of the first Monday in October, and terminating it in the middle in place of the end of March, and postponing the examinations until after the lectures have closed. A preliminary term of two weeks is also to be given, "devoted to subjects which, though practically useful to the student; cannot, for want of time, be treated in sufficient detail in the ordinary course." In the speech of Prof. Miller, to which we have alluded, he gave specimens of these "practically useful" preliminary lectures. His hearers will not soon forget the account he gave of those lectures; lectures too, which he heard delivered. We forbear detail. Let us push forward.

The arguments thus adduced in favor of the five months course are cogent; but why are they urged now? Why have they lain dormant for the past three years? If the term was but of *five* months length, why so often and so publicly assert that it was full *six* months, and was working miracles among students by being a *six* months course? Was no explanation due to the public on this point? How can any body of men parade such glaring antagonism before intelligent people? The Faculty are now feverish with the idea that the falling off of their classes is due to the long term. After a while they may begin a new series of cavortings that may lay in the shade even those we have just been gazing at in wonder. Time may show the Faculty that even a five months term cannot open the tomb. The age of centralism in medical education, is past, and the prestige of an ancient institution cannot recall it from its grave. The University has now leaped into the territory upon which she recently made war, and has made the duration of her sessions conform to that of other schools. Time and honest catalogues will tell the rest of the story.

We have been thus plain in our remarks, because we felt that the occasion demanded plainness of speech. We felt too, that this was due from us, because no improper motive can be justly suspected in us. We neither belong to, nor have any, the least connection with any Medical School. For this reason our criticism cannot be open to the suspicion which, the circular palpably reveals as the cause of the "modifications" of the lecture term of the Medical Department of the University of Pennsylvania—of self-interest B.

SELECTIONS

FROM

FOREIGN AND HOME MEDICAL JOURNALS.

Fellis Bovini, as a Medicine. By A. Cummings, M. D.—The bile from the gall of the ox has long been known to possess medicinal properties, and to some extent it has been used by the profession as a remedial agent, but I believe it has never gained that confidence among practitioners of which its real value renders it worthy. I have used it somewhat extensively in my practice for a few years past, and in this article I design only to give the result of my own experience with it, and the conclusions to which I have arrived in relation to its real and comparative value. Before proceeding, however, I would remark that the form in which I have almost immediately exhibtied it, is that of pills, made of the inspissated gall, rolled in flour, magnesia, Pulv. Glychyrriza, or some other fine powder, to render them of a suitable consistency. The gall may be evaporated in shallow basins, in an oven, or in the sun, until it becomes sufficiently firm to form into pills as above. This, in my opinion, is far the best method. It may be given in its liquid form, but it is less agreeable to the patient, and if not mixed with proof spirits will soon become unfit for use. The medical properties of this agent, so far as my observation goes, are *laxative, al-*

terative, and slightly tonic. Its most valuable agency is exerted upon the stomach and liver. It seems to combine, in a remarkable degree, the three properties named above, and for many diseases to which the chylopoietic viscera are liable, I have found it a most excellent and valuable remedy. I proceed now to notice its application as a remedial agent in disease.

1st. *Obstinate Constipation.*—I am well aware that this is much oftener a symptom of disease, than disease *per se;* but I notice it in this place in order to speak of it in that form so common to those engaged in sedentary and confined occupations, and especially of females in large cities, amongst whom, for want of proper exercise and care, constipation is so prevalent and detrimental. In cases of this description, I have seldom, if ever, been disappointed of obtaining relief by the use of the agent under consideration. The hardened, compact, clay-colored fæces so common in cases where there is more or less obstruction of the liver, are, by the use of the gall, broken down in the intestines, and rendered so friable as to be easily discharged. This result is procured, not only by the combination of the gall with the hardened concretions, rendering them soft and unirritating, but also by removing the obstruction, and permitting the natural flow of bile from the gall-bladder into the intestines. Thus it answers, in this respect, a two-fold purpose. Any one who doubts the efficacy of this remedy, by pouring a few drops of fresh gall upon hard clay-colored fæces, and observing how soon the mass becomes liquid, cannot fail to be convinced. It imparts a healthy tone to the bowels, and promotes the natural secretions which may become impaired.

2d. In *bilious diseases*, arising from a torpid action of the hepatic function, the gall is an excellent remedy. It seems to act as a stimulant to the liver, and promote the secretion of the bile, and also to cause it to flow freely into the bowels, and thus accomplish its normal functions in the animal economy.

3d. In *Jaundice*, you will find that the exhibition of gall, if continued sufficiently long, even in small doses, will not fail to accomplish a desirable and satisfactory purpose. I could relate the history of many cases in my own practice, in which there was every symptom of this

disease, where the gall has acted in the most satisfactory manner. As a stimulant to the liver, I generally prefer it to the blue pill, or mercury in any form, though there may be chronic diseases in which the mercury or some other alterative would be preferable. Whoever, at least, will thoroughly test the powers of this agent, I am confident, will find that I have not exaggerated its value. At least I am willing to abide by the judgment of others who may test it, as to the truth of my assertions. It may be necessary to continue the exhibition of this remedy for some length of time in several cases of jaundice, but it is perfectly harmless, and moreover it may be used in those cases in which mercury cannot be administered, on account of the prejudice of patients or their friends against it, or of any idiosyncracy of constitution, where its use may be interdicted. At least it is a most valuable auxiliary.

4th. In *Dyspepsia*, also, I have in many cases seen the most gratifying results from the use of this remedy. It seems to impart a good tone to the stomach, and by its laxative effects upon the bowels, as well as by its soothing the irritated mucous surfaces of the stomach, proves, in my hands, at least, an excellent remedy. It leaves the influence of a mild tonic bitter on the stomach, not sufficient, however, to produce pain; and its laxative effects in dyspepsia cannot be but beneficial, for, in most cases in this disease, the bowels are torpid, and not unfrequently obstinately constipated. As a remedy also collaterally,

5th. In *Hemorrhoids and Prolapsus Ani*, the gall is justly entitled to our consideration and confidence. If it has no direct or specific influence in removing these forms of disease, it is at least one of the best laxatives in the general torpor of the bowels which accompanies them, since it not only evacuates, but soothes the bowels, and does not produce the irritation in piles and prolapsus ani that most other articles of the class do. But I am also inclined, from my experience with the article, to believe that it exerts a very favorable influence, at least, in the cure of these troublesome and painful affections. At least it justly merits a fair trial.

6th. In *Bilious and Intermittent Fevers* the gall cannot but exert a favorable influence, since its office is not only

to act as an alterative, and rouse the liver to its wonted action, but also to carry off from the bowels the superabundant bile, and to give tone to the chylopoietic system. In a word, in all those forms of disease, (and they are many,) arising from torpor of the hepatic system, I believe that there are few medicines that will give equal satisfaction with the one under consideration; and it is certain that no remedy is more safe in its administration and effects.

One more tormenting and dreaded effect arising from bilious derangement, in which the gall acts in a very favorable manner, I had almost forgotten to mention.

7th. *Sick Headache*, so called, though not strictly speaking a disease, is a sympathetic, symptomatic affection, of very frequent occurrence, and always excruciating, and dreaded by those who are subject to it periodically or occasionally. As it arises from a bilious state of the stomach, the gall, given in small doses, and as frequently as is necessary, seldom fails to mitigate the symptoms, or entirely to relieve, or prevent its accession. It should be given in periodical cases for a season of at least a day or two before the anticipated attack.

8th. In *Typhus and Typhoid Fevers* it is an excellent laxative, where strong cathartics are not required, and will be found worthy of confidence whenever a remedy of the class seems to be indicated. Also the low forms of

9th. *Nervous and Continued Fevers*, no better laxative, in my judgment, can be found, since in those cases strong cathartics are almost invariably contra-indicated. But I need not particularize further, since I believe enough has been said to give my ideas in relation to the class of cases in which this remedy is indicated; and as I cannot expect to gain the confidence of practitioners without their first giving the article in question a fair and impartial trial, I have perhaps already written too much. I am confident, however, that those who make a fair trial of it will not accuse me of exaggeration, for I have endeavored candidly to give the value of the article as it has proved itself in my own practice, and not from theory deduced from the natural properties of the medicine. I have said it is necessary, not unfrequently, to continue the medicine for some length of time, in obstinate cases, especially. But

it is harmless and safe, and will act well in any constitution, and is contra-indicated by no form of idiosyncracy. The dose of the inspissated gall in the form of pills, or otherwise, is from five to ten grains or more, repeated every two or three hours for a cathartic, and less for a mere laxative effect. It may be given in sufficient doses, at any time, with perfect safety to adults or children.—*Boston Med. and Surg. Jour.*

The Radical Cure of Reducible Hernia, by Injection. By John Watson, M. D.—I do not propose, just now, to go into the whole merits, or demerits of this operation, a task which has been assigned to other hands, and which has already been in some degree achieved, by one of the committees of the American Medical Association (see their report in the Transactions for the session of May last.) My only object is to give the details of a single case treated in this way, in connection with such hearsay information as I have been able to collect concerning the operation in other quarters.

The procedure now under consideration, if I am not mistaken, was first brought into notice through an irregular channel, by a certain Industrialist, of New England. But the first notice I remember to have seen of it, was in Professor Pancoast's *Operative Surgery.* In July, 1848, my attention was for a second time called to this subject, by a gentleman under my care for the treatment of varicocele, who, a year or more previously, had undergone an operation for the cure of a reducible hernia, by what he described as a trifling process, and with complete success. He spoke of it as a simple puncture, which subjected him to very little uneasiness; and assured me that he was cognizant to the cure or relief of other individuals, who had been treated like himself, by a practioner of Boston. I subsequently ascertained that the instrument with which this individual operates, had been prepared by a cutler of this city; but on inquiry I found that it had been patented by the operator, and was, consequently, to be used only by himself. As I had made inquiry for it, with the view of employing it on a case then in hand,

could not but feel indignant that any practitioner claiming to belong to the regular profession, should have thus prostituted his noble calling to mercenary ends; but believing that no special form of patent instrument is essential for the making of a puncture, or for introducing an irritating fluid beneath the integuments through this, I attempted in my own way to get along without it, as in the following case:

Joseph A. Seavell, of Ohio, seaman, aged 31, was admitted into the New York hospital, Nov. 24, 1851, with a large inguinal hernia, occupying the left side of the scrotum, which had been then protruding for several hours, and had resisted several well-directed efforts for reduction. The patient for the last four years had been occasionly troubled by the protrusion, but had never before been baffled in his efforts to reduce it; and by the use of a truss he had been able to follow his regular occupation. With some little trouble the tumor was reduced by taxis, soon after his admission; and on the 29th of November, having explained my object to the patient, and obtained his consent, I attempted to effect a radical cure of the hernia.

While the patient was lying on his back, with his scrotum and left spermatic cord drawn slightly towards the right side, and with the integuments over the left external abdominal ring slightly on the stretch, I introduced the point of a delicate bistoury through the integuments, directly down to the crest of the os pubis, the point of the instrument touching, without dividing, the lower termination of Poupart's ligament, and made to work freely in the loose tissue immediately in front of the ring, but without wounding the spermatic cord. Having made the puncture, and withdrawn the bistoury, the nozzle of a small syringe charged with tincture of cantharides, was introduced through the wound, and about a drachm of this fluid was injected into the bottom of the cut, the hand of an assistant, in the meanwhile, resting firmly over the inguinal canal to prevent any portion of the injected fluid from entering this, or passing through the sac into the abdomen.

The whole procedure was the work of a few seconds, and gave the patient little or no uneasiness. I next applied a compress and spica bandage, to keep the parietes

of the inguinal canal in close apposition, and administered an anodyne, keeping the patient on his back, with directions to apply an evaporating lotion, should severe inflammatory symptoms supervene.

In a few minutes after the operation, he began to speak of pain from the injection. The sore became more troublesome, and extended for several inches in every direction, but was severest along the ascending track of the spermatic cord. He slept but little during the following night; but next morning the pain had subsided, a slight soreness, only, remaining in the part. The patient was at the same time suffering from chancres. I made the treatment of these the pretext for keeping him on his back with the compress and bandage applied as above, for several days. He spoke of no uneasiness from the operation after the second day. On the 12th of December he was walking about without his truss, and with no apparent tendency to a recurrence of the hernial protrusion. On the following day, being desirous to join his vessel which was about to sail for South America, he requested his discharge, promising to write to me, and report the further progress of his case, should the swelling reappear—and, if possible, to report in person, at the close of his voyage. But, as yet, I have not heard of him.

The operation in this instance had evidently a beneficial effect, and I am not certain that it may not have effected a permanent cure. I am not disposed to believe that any portion of the injected fluid entered the hernial sac; but by exciting inflammation around this within the column of the external ring, and the subsequent condensation of tissues, which is so apt to follow acute inflammation, we can readily imagine that this procedure may, now and then, effect an object we have hitherto sought in vain to effect by other and severer measures.—*N. Y. Medical Times.*

Paste made from Pumpkin Seeds in the Treatment of Tænia. Translated from the French, by S. W. Butler, M. D.—We have often repeated that our indigenous materia medica is not as meagre as is made to appear, and if we employed in its study the care which we bestow upon

that which commerce brings us at great expense, this truth would not need to be repeated. Here is an example. It is more than thirty years since the *Journal Universel des Sciences Médicales* described the good effects which Dr. Mongeny had obtained in cases of tænia with a paste composed of ninety grammes (about 3½ ozs.) of fresh pumpkin, and double that quantity of honey, given in three doses, an hour apart. By this means, this physician had constantly succeeded in dislodging the tænia in the course of six or seven hours, even, in many cases after all the remedies then known had failed.

This result was sufficiently important, and the innocuity of the medicine incontestable enough to have provoked an extended trial, but observations upon it have only been made by the practitioners of Bordeaux. In 1845, Dr. Brunet pointed out to his colleagues of the Medical Society, the remarkable results which he had obtained by the use of pumpkin seeds, and since that time many of his brethren have cited numerous cases of expulsion of tænia by the employment of this remedy.

At Bordeaux, the paste is prepared with forty-five grammes (about 1¾ oz.) of the hulled seeds of the large pumpkin (*cucurbita maxima*) with the same quantity of sugar.

Here is another proof of the success of this remedy. A physician suffered almost constantly with pains in the lumbar region, and with general lassitude; the least labor fatigued him greatly. This state had discouraged him, and made him fear an affection of the nervous system.—Having perceived that he discharged living white worms, resembling pieces of flattened dog's grass, (*chiendent aplati,*) of the length of an inch, or nearly, and although he distinguished in them a perforation, and not the slightest trace of articulation, he thought himself affected with tænia. In accordance with the advice of M. Saramea, he took, at eleven o'clock at night, thirty grammes (about an ounce) of pumpkin seeds, mixed with one-third the quantity of sugar. The next morning—twelve hours after—having taken a simple injection, this physician passed seven metres (a little more than seven yards) of tape-worm. We cite this case simply to set forth the efficacy of the means employed; and we must here mention that, in the discussion brought forth by this fact, Dr.

Brunet said he had employed this remedy twenty-five or thirty times with success since his first communication to the Society. M. Saramea has also succeeded in a great number of cases, but he has been obliged sometimes to employ the remedy a second or even a third time. This physician cites two cases in which the root of the pomegranate, and even kousso, had failed. It is not necessary to believe, however, [in regard to the remedy in question,] that no instance of failure may occur. M. Brunet and M. Costes have pointed some out, and they have asked, with just reason, if these various results may not be consequent upon the species of tænia treated. This remedy has seemed sufficiently efficacious to warrant the Society in entertaining the proposition of one of its members, to address a note to the National Academy of Medicine, with the request to order its insertion in its proceedings, and ultimately in the *Codex*.

This communication will, we doubt not, be received favorably by the Academy, and we would hold it up as an example worthy of imitation—as occasion offers—by other societies!—*N. J. Med. Reporter.*

Treatment of Hooping Cough.—In Hooping Cough, Dr. Madison, of Virginia, recommends a blister to the nucha; Dr. Golding Bird uses *conium*, in the following form: ℞ Ext. Conii, grs. xij; Alum, grs. xxv.; syrup papaver, ℥iij; aq. fœniculi ℥iij; M. A desert-spoonful every four or six hours. Dr. Spengler, in a French journal, recommends *conia* in doses of one-fortieth of a grain for children three or four months old, and one-tenth of a grain for children of as many years. He has employed it in several cases successfully.

Strangulated Hernia in a Child five months old; Operation; recovery. Under the care of Mr. Erichsen.—The following particulars were obtained from the notes of Mr. Turle, Mr. Erichsen's house-surgeon:

Arthur C——, aged five months and a half, was admitted April 12, 1852, under the care of Mr. Erichsen. It appears that when the boy was three weeks old, a hernial tumor appeared in the right groin; it could be easily reduced, and would only come down occasionally—viz: about once a week. The tumor then used to remain apparent for several hours, and afterwards ascend spontaneously into the abdominal cavity. When the child was four months old a truss was applied, but the apparatus proved ineffectual, as the tumor appeared again on the following day by the side of the pad. The truss seemed also to cause considerable pain, and the mother, therefore, brought the child to the hospital.

The little patient was found, on examination, to be affected with oblique inguinal hernia of the right side; the intestine had descended into the scrotum, and was greatly distending it. There were, however, no symptoms of strangulation at the time; some attempts were made to reduce the tumor, but these having failed, the mother was desired to leave off the truss and to bring the child immediately she perceived any unpleasant symptoms.

Six weeks after this (April 12, 1852) the mother applied to Mr. Erichsen again, as alarming signs of strangulation had manifested themselves. The tumor, which had remained unreduced, was now large and tense, its neck appearing to be tightly constricted by the external abdominal ring. In following the intestine down into the scrotum, it was remarked that the latter was not only distended by displaced intestine, but likewise by fluid secreted in the cavity of the tunica vaginalis, so that the child was suffering both from strangulated inguinal hernia and hydrocele. Constipation and vomiting had been existing for some time, the surface was cold, the face pale and drawn, and the patient evidently in a very precarious state.

The mother, having been told of the dangerous condition of her child, readily consented to the performance of the operation, and Mr. Erichsen proceeded, as soon as the child was narcotized by chloroform. The integuments

were divided over the neck of the tumor, and the several layers of cellular tissue and faciæ having been carefully slit open, the sac was fairly exposed. The constriction was now found to be exerted by the external abdominal ring, and a few transverse fibres of the cremaster muscle; a curved director was passed beneath these parts, the strangulating structures were divided by an incision directly upwards, and the protruded intestine returned with facility without the peritoneal sac having been opened. The edges of the wound were approximated with sutures, and the whole secured by a compress and a double-headed roller.

Three hours after the operation a copious liquid motion was passed, the child being perfectly quiet and composed. On the following day he was in a very satisfactory state, and the wound looked very healthy. From this time the little patient progressed most favorably; he had no bad symptoms whatever, became cheerful, and improved much in appearance. The bowels were open daily, and the child left the hospital, the wound being quite cicatrized, eighteen days after admission.

It will be perceived that the taxis was not tried at all in this case, when the symptoms were fully established; and that this cautious conduct has a marked and beneficial influence on the issue, is clearly seen by the success here obtained. We cannot help noticing, in reviewing this case, the baneful effects of the common spring truss in infants; nor can it be otherwise, for it is next to impossible that the pad should exactly compress the ring with infants at the breast. It is equally difficult to regulate the spring in such a manner as to prevent the truss from slipping, without using an amount of pressure which must of necessity be hurtful to the child. Nothing will answer in such cases but elastic belts, which yield without becoming displaced when the infant cries; the belt being supplied with an air-pad, as suggested by M. Bourjeaurd, which shall be gently lodged upon the ring, and be made softer or harder, by means of the stop-cock, as occasion may require.—*London Lancet.*

Prevention of Salivation. By George Stearns, M. D.

I wish to communicate a fact to you that has recently fal-

len under my observation, which may be of some interest to the profession generally. All physicians are aware of the salivating effects of calomel, and of the inconvenience that arises from sore mouths and other irritating complaints that affect the patients.

I have had several persons under my care to whom I have been obliged to administer calomel, which I have mixed with supercarbonate of soda, in the proportion of about twice the amount in weight of soda. To one patient in particular, whom I have attended for about ten weeks, I have given three grains of calomel with six grains of soda daily for five weeks, besides administering it frequently during the rest of the time. As yet he has not suffered at all from the salivating effect of the calomel, which has nevertheless been very beneficial to him. It is possible that these were all persons not susceptible to salivation? Or is the absence of salivation to be attributed to the supercarbonate of soda?—*Boston Medical Journal.*

A Case of Poisoning with Oil of Tansy—Death at the end of three hours and a half—Quantity of the Drug taken about ℥j *and* ℥iii. By John C. Dalton, Jr., M. D. (Read to the Boston Society for Medical Observation, June 2, 1851.)—E. S., a fine, healthy-looking girl, about twenty-one years of age, died at the house of Mr. A., in Boston, on Wednesday, the 7th of May, 1851. She had been employed in Mr. A.'s family as a seamstress since the previous winter, living in the house during the week, but going away on Saturdays to a cousin's in Pleasant street, and returning to Mr. A.' son Monday morning. She had been, for some months, receiving the attentions of a young man who was reputed to be engaged to her. None of her friends, however, suspected anything to be wrong with her until Monday, May 5, when her cousin, with whom she had been spending Sunday, as usual, perceived the odor of tansy in the room which she had occupied; whereupon it occurred to her that the girl might have become pregnant, and used the drug for the purpose of producing abortion.

On Tuesday, she was engaged in her ordinary employment, and dined heartily a little after five o'clock in the

afternoon. She went up stairs to her room about half-past nine o'clock. The cook, who occupied a room above, went up with her and stopped in her room, conversing for some fifteen minutes. The girl's manner was perfectly natural and cheerful, as it had been throughout the day. About a quarter before ten o'clock the cook left her preparing for bed, and went up to her own room.

Nothing more was heard from her till about eleven, when Mr. and Mrs. A., who were sitting in the basement-room, heard a scream, which they supposed to come from one of the children. Mrs. A. went immediately up stairs, and on entering Miss S.'s room, found her on the floor, by the side of the bed, insensible and in violent convulsions. She had evidently fallen out of bed, as she was undressed, and the bedclothes were disturbed, and had been partially dragged on the floor with her. Dr. Morrill was immediately sent for, and arrived in about ten minutes. He sent also for me, and I arrived at the house at half-past eleven o'clock.

The girl was then lying on her back by the side of her bed, and presented the following appearances: Total unconsciousness; cheeks flushed; of a bright, red color; eyes open and very brilliant; pupils of equal size; widely dilated and immovable; sclerotics injected; skin warm, not remarkable as to moisture; respiration hurried, labored, stertorous, and obstructed by an abundance of frothy mucus, which filled the air-passages, and was blown from between the lips in expiration; the breath had a strong odor of tansy, as had been already observed by Dr. Morrill; pulse quite full, forcible, 128; at intervals of five to ten minutes the body was convulsed by strong spasms, in which the head was thrown back, the respiration suspended, the arms raised and kept rigidly extended, and the fingers contracted. After this state of rigidity had continued for about half a minute, it was usually succeeded by a tremulous motion, often sufficient to shake the room, together with very faint and imperfect attempts at inspiration. The whole interval from the commencement of the convulsion to the first full inspiration, varied from a minute to a minute and a-half. Occasionally, the tongue was wounded by the teeth, and the saliva slightly tinged with blood. Immediately after a convulsion the countenance was very pallid and livid, from the

suspension of respiration, and the pulse exceedingly reduced in strength and frequency. The pulse and color then gradually returned until the occurrence of the next spasm. It was very common, a few seconds after the termination of a convulsion, for the head to be drawn slowly backward, and the eyelids, at the same time, stretched wide open. In the intervals of the convulsions the limbs were mostly relaxed, but the jaws remained clenched.

A vein was immediately opened in the right arm, and about Oij of blood taken away. After this, the pulse became much softer, and the face lost its bright color. There was, however, no change in the condition of the pupils, nor return of consciousness, nor other improvement in the appearance of the patient. It being impossible to get anything down the throat, two injections of an ounce of wine of antimony, with about ℥ss of powdered ipecac., were thrown up the rectum at intervals of about half an hour, but produced no apparent effect.

On searching the room, a ℥ij phial was found in the pocket of the girl's dress, wrapped in a piece of paper, labelled "Oil of Tansy," and marked with the name and address of an apothecary in Pleasant street. The phial contained ℥v of the oil of tansy of the ordinary purity.—A mug was also found from which she had apparently drunk the oil, mixed with water, as it smelt very strongly of the drug, and still had a drop or two of it at the bottom.

The condition of the patient continued much the same for about an hour. The convulsions, however, gradually became less protracted, and the failure of the pulse after each attack, more complete, at the same time that it recovered strength less perfectly in the intervals. The countenance also became somewhat sunken and the temperature of the skin reduced. About 1 o'clock, six leeches were applied to the forehead and temples, and sinapisms put on the calves of the legs. The leech-bites bled freely.

Toward two o'clock the alteration for the worse became quite rapid. Pulse 124 and feeble; respiration 36, and attended with less muscular effort than at first; the left cornea was glazed, but the right continued brilliant; a little inward strabismus of the right eye, and the mouth

and nose drawn a little to the right side. Occasionally a slow, lateral, rolling motion of the eyeballs. At five minutes past two she had the last convulsion, which was much less violent than the earlier ones, and lasted only half a minute. There was no recovery of the pulse after this attack, and she died at a quarter past 2 o'clock, A. M.

The present case is another instance of the extreme violence to which the system may be subjected even in the early months of pregnancy, without inducing abortion. Though all the muscles, both of the body and limbs, were for three hours and a quarter subjected to a succession of the most violent contractions, there was no sign of abortion, and after death the ovum was found in the uterus entirely undisturbed. In Dr. Hildreth's case, also, pregnancy existed but a few weeks advanced, and the drug was undoubtedly taken for the purpose of producing abortion, but nothing of the kind took place. The general symptoms in that case were similar to those described in the foregoing, the most remarkable difference being the more gradual loss of consciousness, and the more rapid death after a much smaller dose.—*American Jour. Med. Sciences.*

On the influence of Poisons upon Animal Heat as a Cause of Death. By E. Brown Séquard, M. D., of Paris. Prévost and Chossat, and after them, M. Magendie, have ascertained that death occurs quickly in mammals when their temperature is notably diminished. My experiments confirm the correctness of that statement. The diminution of animal heat in mammals is so dangerous, that in one case I have seen death take place in a rabbit after a diminution of only 22° F. (12° Cs.) I have never observed any animal continuing to live when I had diminished its temperature more than 44° F. (24°.5 Cs.) I have found the law established by Chossat perfectly correct, according to which the diminution of animal heat necessary for killing is less and less, in proportion to the rapidity with which that diminution takes place.

It is very probable that in all the cases where, in consequence either of disease, or of a wound, or of poison, the temperature of man is diminished many degrees, his

life is in danger from the very fact of that diminution. It is thus in cholera, in sclerema, in certain cases of palsy, in cases of great disturbance of the respiration, in fractures and luxations of the vertebral column, in the cervical, and even in the dorsal regions, in cases of profuse hæmorrhage, and in many cases of poisoning when death is not rapidly produced.

It has been long known that temperature is diminished in poisoned persons; and there are but few cases of poisoning on record in which it is not said that the patient was cold. Chossat has found that a dog, into whose veins he had injected opium, had its temperature diminished from 105° to 62.6° F. (40°.3 to 17° Cs.,) 22 hours after the injection. Brodie has found that many poisons act upon animal heat so as to diminish it considerably. Demarquay and Dumeril, junior, and later, these two experimenters, joined with Lecointre, have found the same thing as Brodie in many toxic agents. I have made very numerous experiments on this subject, and some of their results have been published before the last papers of Demarquay, Dumeril and Lecointre.*

I have stated that many poisons, either injected in the veins or absorbed by the vessels of the skin or of the digestive canal, may diminish sufficiently the temperature of Guinea pigs and rabbits to produce death. This occurs when the dose of the poison is not large enough to kill in less than four or five hours. These poisons may kill only by their action upon animal heat. It may be so with opium, cyanhydric acid, the cyanide of mercury, hyoscyamus, digitalis, belladonna, tobacco, euphorbia, camphor, alcohol, acetic, oxalic, sulphuric, azotic, chlorohydric acids much diluted, and some oxalates.

Of course the action of these poisons is the greater, the colder the atmosphere; but it is not always immediately so, and instead of diminishing the animal heat, many may increase it for a time, especially when the temperature of the atmosphere is elevated.

I have discovered that a dose of one of these poisons, sufficient to kill an animal, when there is no obstacle to the diminution of its temperature, may be unable to destroy life when the temperature of the animal is main-

* See Gaz. Med. de Paris, 1849, t. iv., p. 644.

tained by artificial means to its normal degree, or not far from it. My experiments have been conducted as follows: —

Equal doses of poison were given, simultaneously, to two animals, as much alike one another as possible. One of them was left in a room at a temperature of from 46 to 50° F., (8 to 10° Cs.,) and the other was kept not far from a chimney, in a place where the air was at from 75 to 86° F., (24 to 30° Cs.) The first was dead after a certain number of hours, or sometimes one or two days, having its temperature much diminished. The other, on the contrary, had no perceptible diminution of its temperature, and was generally cured very soon. Therefore, when taken in certain doses, many poisons may kill only by their influence on animal heat, physicians, in cases of poisoning, should try as much to prevent the diminution of temperature, as to expel the poison or to act against it by an antidote, by pulmonary insufflation, or otherwise.

In experiments which I have made lately on the action of very pure digitaline, that had been prepared by M. Quévenne, the celebrated chemist, who has made such interesting and accurate researches on digitalis and the substances of which it is composed, I have found that this poison may also diminish temperature. I believe it is easy to explain the contradiction existing between Traube and Stannius, as regards the influence of digitalis on animal heat. When the atmosphere, in which the animal is, is cold, then its temperature may be diminished by digitalis or digitaline, but when it is warm the diminution does not take place, or it is very small. But, of course, if the dose is sufficient to kill very quickly, then it is indifferent whether the atmosphere is cold or not, because there may be not time enough for the diminution of the temperature of the animal.

I have to relate another fact which, I believe, ought to be considered as analogous to the preceding. It is that kind of poisoning which occurs when a layer of oil, of varnish or of gelatin, put on the skin of a warm-blooded animal. Death, then, is very probably produced by a substance unknown until now, and which is secreted by the skin. The layer of oil, varnish, or gelatin preventing that secretion taking place, that unknown substance

becomes accumulated in the blood, and then are produced the phenomena so well studied by MM. Fourcault, Becquerel, Breschet, and Magendie. I have found that in such a case the animals may live, if the atmosphere in which they are kept is at a temperature inferior to 79 or 80° F., (26 or 27° Cs.) In these circumstances their temperature is not sensibly diminished, while it diminishes much when the atmosphere is cold. Therefore it is especially by their loss of warmth that animals are killed, when their body has been entirely covered with oil, varnish, or gelatin.—*Ib.*

On the Normal Degree of the Temperature of Man. By E. Brown Séquard, M. D., of Paris. The degree of animal heat, in the human species, is stated as being between 98.5° and 100° F., (37 and 38° Cs.) I intend to prove that it is higher.

It is impossible to take directly the temperature of most of the internal parts of the body in man; therefore many physiologists, in order to discover the temperature of these parts, have argued as follows: According to J. Hunter, the temperature of the rectum in animals is the same as that of the right ventricle of the heart; hence it has been concluded that as the temperature of the rectum, in man—still according to Hunter—is 98°.42 F., (36°.9 Cs., the temperature of the internal parts of the body ought to be between 98 and 99° F., (36°.67 and 37°.22 Cs.)

Some other physiologists, noting the temperature of the mouth under the tongue, and supposing that this temperature is nearly the same as that of the internal parts of the body, have concluded that the temperature of man is between 99 and 100° F., (37°.2 and 37°.8 Cs.)

We will prove that these deductions are not right:

Firstly, the degree of the temperature of the rectum is not, as Hunter says, 98°.4. It may be so in debilitated men, but in healthy persons it is more elevated. It is between 100 and 102° F., (37.7 and 38°.89 Cs., according to my own researches and to those of Berger and Maunoir. Besides, many experiments on dogs, rabbits and Guinea pigs, have shown satisfactorily to myself that

the temperature of the rectum is not equal to that of the right ventricle. This last organ is from 1 to 3° F., (0.55 to 1.7° Cs.,) higher than the rectum.

The temperature of the rectum, in man, being between 100 and 102° F., (37°.7 and 38°.8 Cs.,) and the temperature of the right ventricle, in man, being from 1 to 30° F., (0.56 to 1.7 Cs.,) higher than that of the rectum, if we suppose the same difference existing in man as in mammals, it follows that the temperature of the right ventricle of the heart in man ought to be between 101 and 105° F., (38°.33 and 40.56 Cs.)

But a closer approximation of the exact temperature of the internal parts of the body, in man, may be obtained by taking the temperature of an organ situated deeper than the rectum, and, consequently, less exposed to the influence of the atmosphere. Such is the case in the bladder. I have observed its temperature by taking that of the urine at the moment of its emission, and before any sensible change had occurred in it. The urine was directly received in a vase dipped into a large quantity of water at 98° F., (36°.7 Cs.) Thus I have ascertained that the mean temperature of the urine, in man, is 102°.6 F., (39°.2 Cs.)

Now if we take notice of this well known fact, that the temperature of the lower part of the abdomen is less elevated than the upper part, we are authorized to believe that the temperature of the central parts of the body, in man, is very near 103° F., (39°.5 Cs.)

My experiments on the temperature of urine were made on ten strong sailors, in the spring, on the Atlantic Ocean, between the 43d and 45th deg. of north latitude. The lowest degree of the temperature of urine which I have observed, was 100°.9 F., (38°.3 Cs.;) the highest was 103°.2 F., (39°.56 Cs.) My own urine, examined more than thirty times, in the most varied conditions, has been nearly always at the same temperature; the variations have been only between 102° and 102°.8 F., (38°.89 and 39°33 Cs.) The ordinary degree is 102°.5 F., (39°.17 Cs.)

Long before my researches, the temperature of urine, in the human species, had been taken by some observers, but in general without sufficient care. The temperature of the urine is 94°.25, according to Braun; 98°.9, according to De Lisle; and 103°, according to Hales. Recently

Berger has taken the temperature of urine in the bladder in five women. He has found it equal to 101°.48 F., (38°.6 Cs.)

As the temperature of woman is a little inferior to that of man, the result obtained by Berger is in accordance with mine.

If we take the average between the temperature of the urine of man as I have found it, and that of woman as Berger has found it, we have a number very near 102° F., (38°.9 Cs.)

From these facts we draw the conclusion that the temperature of the thoracic and abdominal viscera, in the human species and in both sexes, is between 102 and 103° F., (38°.89 and 39°.44 Cs.,) *i. e.*, some degrees higher than it is generally admitted.—*Ib.*

On the Influence exerted upon the General Temperature of the Body by a change in the Temperature of one of the Extremities. By E. Brown Sequard, M. D., of Paris. The following sentence is given, as an axiom, by Dr. W. F. Edwards: "We cannot either raise or lower the temperature of any one part of the body, without all the other parts of the frame being affected and suffering a corresponding rise or fall in temperature, more or less, according to circumstances."*

Expressed in such terms, we accept this law as perfectly true. But the author elsewhere gives an extension to this law, which we will prove to be incorrect. No doubt when the temperature of the blood, coming to the heart from a remote part of the body, has been modified in that part, the thoracic viscera ought to have their temperature modified; but to what extent? It is on this very important point that we do not agree with Doctor Edwards. He was of opinion that the influence exerted by a small part, on all the other parts of the body, was considerable. He says that the chilling of a single part, such as the hand or the foot, may cause a loss of temperature in all the other parts of the frame, even far beyond

*Article Animal Heat, in Todd's Cyclop. of Anat. and Physiol 1839, t. ii., p. 660.

what could have been presumed as likely or possible, and that in a number of experiments where one hand was plunged in water cooled down by ice, the other hand, which was not subjected to the action of the cold bath, lost nearly 5° R., (11°.25 F., 6°.25 Cs.,) in temperature.

It will be easily understood how important it would be that practitioners should be able to act on the general temperature of a patient, so as to increase or diminish it, by means of a hand or a foot bath. I regret to say that if the facts observed by Edwards are exact, his conclusion nevertheless is incorrect.

I have performed, both alone, and with the help of Dr. Tholozan, Physician of the Hospital *Val-de-Grace*, at Paris, numerous experiments, with the view of knowing whether Dr. Edwards was right or not. I have found that the chilling of one hand plunged in water at the temperature of freezing point, acted very strongly on the temperature of the other hand. But, at first, there is no regularity at all in the quantity of degrees of temperature lost by the hand which remains out of the water; and secondly, we have found once that this hand did not lose any fraction of its temperature. In one case we have observed that the hand kept in the atmosphere did lose 22° F., [12° Cs.] in seven minutes. The ordinary loss of temperature has been of between 6 to 8° F., [3.33 to 4°.44 Cs.] In one case there has been no loss, and, on the contrary, there has been an increase of temperature of 1°.4 F., [0°.8 Cs.]

If, like Edwards, we consider the loss of temperature of the hand not plunged in water as a sign and as a measure of the diminution of the general temperature of the body, we must conclude, from our experiments, that the chilling of a small part of the body may be unable to diminish the temperature of the body sensibly, and that, in other cases, it may act extraordinarily upon it. But the supposition that the hand kept in the atmosphere was able to give a measure of the modification of the body, is incorrect. By taking the temperature of the mouth during the time that one hand was dipped into very cold water, Dr. Tholozan and myself have ascertained that the temperature of the body does not sensibly change. The greatest diminution of the temperature of the mouth has been nearly 1° F., [0°.6 Cs.,] and this only in one

case. In that experiment in which the hand not plunged in water lost 22° F., [12° Cs.] the temperature of the mouth was not diminished more than the fifth of a Fahr. degree.

As it is quite certain that the temperature of the body is constantly changing, it is easy to understand why we do not find a small but a constant diminution in the temperature of the mouth when one hand is subjected to a notable chilling. When, under the unknown influences that are constantly modifying the temperature of the body, there is a tendency to increase that temperature, then the tendency to its diminution originating from the chilling of one hand may exist without producing any effect. We have there two causes acting in opposite directions, and if they are equal they annihilate each other. If they are unequal, we perceive only the difference between them. When these two causes act in the same direction, then their efforts are added to one another, and it is probably in a circumstance of that kind that I have once found a diminution of 1° F., [0.°56 C.,] taking place in the mouth.

We have now to examine how the diminution of the temperature of a hand is produced when the other hand is dipped into cold water. *A priori* it is evident that the chilling of the hand kept in the air exists in consequence, either of the arrival of a cooler blood, or in the diminution in the quantity of blood. That hand being exposed to a cold air (for it is only in such a case that the experiments succeed,) loses its temperature by the action of that cold air. At first the blood that arrives in the hand is not cooler, as Edwards had supposed it was. This we prove by the fact that the temperature is but very little changed in the mouth. The supposition then remains that the quantity of blood arriving in the hand is smaller than usual. This may happen by two modes, one of which is that the heart sends less blood, and the other that the blood-vessels of the hand are contracted and prevent, in part, the passage of blood. It is certain that the heart continues perceptibly to send the same quantity of blood. Therefore we are induced to admit that the hand's blood-vessels are contracted. But now what is the cause of that contraction? We will try to show that it is in an action of the nervous system. Every one knows

that under the influence of a sensation or of emotion, the hands, and sometimes the feet, become cold. The nervous system, in consequence of that sensation or emotion, acts upon the blood-vessels and excites them to contract. The calibre of the visible vessels is sensibly diminished. The same phenomena take place in the two hands when one is dipped into very cold water. An exceedingly violent pain is felt, the nervous centres are strongly excited, and they act then as under the influence of an emotion. Dr. Tholozan and myself have observed that the greater the pain felt, the more the temperature was diminished in the hand left in the air.

As to the influence of partial heating Dr. Edwards relates the following experiment:

"The hand being immersed in water heated to the temperature of 34° R., [108°.5 F.—42°.5 Cs.,] rose one degree of the same scale, and the temperature of other remote parts not immediately exposed to the influence of heat were found to have risen to a corresponding degree."

I have repeated this experiment of Dr. Edwards, and I have found no evident elevation in the temperature of remote parts, as the mouth and the hand, not immersed in water.

I conclude then, from the facts contained in this note, that, in general, the temperature of the body is not sensibly modified by the chilling or the heating of a small part of the frame.—*Ib.*

Case of Conception before the Appearance of the Menses. By Wm. T. Taylor, M. D., Philadelphia.—The general experience of the medical world has established it as a physiological fact, that conception cannot take place prior to the appearance of the menses. But there are instances of females becoming mothers who have never menstruated; they, however, must have conceived just at the time when the catemenia were about to be established, which after parturition and suckling probably occurred regularly.

The following case, which I have met with, seems to be an anomaly in the annals of midwifery:

During the month of June, 1851, I was requested to

visit Hannah B , a mulatto, who was pregnant with an illegitimate child. Was much surprised at finding my patient herself was a mere child in appearance and manner. She was thirteen years of age on the 3d of the previous February, and, though she had never menstruated, was, when I saw her, in the eighth month of gestation. Her general condition was plethoric; her breasts well developed, and the areola quite dark.

On the 13th of August she was taken in labor, but in consequence of a prolapse of the funis umbilicalis, which could not be replaced, I delivered her of a still born child, of the usual size, and perfectly formed. The lochia continued for a few days, and she passed through the accustomed period after delivery very favorably.

It is now one year since, and her menses have not yet made their appearance, nor has there been any vicarious discharge; her health during the whole time remaining perfect.

Never having read of a case of the kind, I have sent it to you for publication, should you think it of sufficient interest.—*Ib.*

Extraordinary Case of Hydrocephalus.—A post-mortem examination was made, (by order of the coroner in London, on the 5th of Sept. 1852,) of the body of Joseph Devine, aged fourteen years, found in the Regents Canal, having fallen into the water while being attacked with a fit. The head measured twenty-seven and a quarter inches in circumference; seventeen and a half inches across the top from ear to ear; nineteen and a half inches from the nape of the neck up to the centre of the back, over the crown to the junction of the eyebrows; eleven and three-quarter inches from one parietal bone to the other. The skull was as thin as that of a child two years old, and the sutures were open like those of an infant, never having closed. When punctured, upwards of five imperial pints of water escaped from it, and the substance of the brain itself weighed three and three-quarter pounds. With the exception of the celebrated case of Cardinal, who lived to the age of thirty-two, is the largest head on record.—*Ib.*

THE WESTERN JOURNAL.

THIRD SERIES.—VOL. X.—No. V.

LOUISVILLE, NOVEMBER 1, 1852.

THE KENTUCKY STATE MEDICAL SOCIETY.

The readers of this *Journal* will remember that we gave an account, last year, of the formation of a State Medical Society, in Kentucky. Last year the Society was formed, and, after organizing, and preparing work for a future meeting, it adjourned to meet in Louisville on the 20th October, 1852. The recent meeting, therefore, may be regarded as the first meeting of the Society, and we are sure that we express the feelings of all the members when we say that it is scarcely possible that a more pleasant meeting of the kind ever was held. Every one seemed to be animated by one feeling, and that was devoted to the success of the cause. The physicians of Louisville united themselves in numbers with the Society, and every thing was conducted with the greatest harmony and good will. We do not remember ever to have seen a larger amount of business completed by any Society in the same space of time.

When the Declaration of Independence was announced, it made no change in the actual state of the contest with Great Britain. It announced a position; it proclaimed a resolution, a determination to

accomplish a result; but how far that result lay in the future, it did not proclaim nor indicate. But it accomplished some great ends. It made those who were engaged in the lingering and disputed struggle feel that they had a country; that her head was erect among the nations, and that all who were engaged in the cause were many degrees above the character of mere rebels. In a miniature form these must be the feelings of those who witnessed the sessions of the recent meeting of the State Medical Society in this city. The physicians of Kentucky must rejoice to know that they have a rallying point now for professional advancement, and professional honor. All must now feel that the medical men of Kentucky have harnessed themselves for their duties to humanity, to science, and to their profession. These were the feelings that animated us as we looked upon the proceedings of the Society, and they were strengthened by all that we saw. It was an animating scene to look upon those who had attained the highest positions in their profession, as well as those who occupied humbler places, working sturdily together towards a common point, and we felt that the glory, honor and dignity of a profession, which, in the expressive language of the Louisville Chancellor, "is, at least, next to the profession of the legate of the skies," were in hands that would protect them. May the spirit of labor, and devotion to the interests of medical science, that moved the members of the Society at its recent meeting, ever characterize the movements of the association, and thus advance alike the interests of the profession and of society.

As a matter of interest to the profession, we publish a synopsis of the proceedings of the Kentucky State Medical Society, at its recent meeting in this city.

At the second annual meeting of the State Medical Society, held in the Circuit Court Room, in Louisville, on the 20th day of October, 1852, at 1 o'clock, P. M., the President, Dr. Wm. L. Sutton, took the chair, and called the Society to order.

Dr. J. B. Flint offered an amendment to the second section of article three of the Constitution. The amendment is for the purpose of increasing the facilities for admitting members. It was not acted upon at this session, and lies over for consideration next year.

The roll of members was called, and a considerable proportion of the original members answered to their names.

A large number of new names was offered for membership, and, during the progress of the session, some seventy new members were enrolled.

AFTERNOON SESSION.

The Committee appointed to report on the order of proceedings, reported a series of recommendations. which was adopted. One of the series, which provided for the nomination of the officers of the Society by a committee, was afterwards stricken out.

On motion of Dr. Gross, the thanks of the Society were given to the Indiana State Medical Society for copies of the proceedings of the third annual meeting of that Society, and, upon motion of Dr. L. P. Yandell, Drs. Clapp, Leonard and Steele, of New Albany, Ia., who were present, were invited to take seats with the Society, and participate in the proceedings.

Dr. Bell presented the form of case-book modelled by the President of the Society, which he moved to refer to a committee to report upon. Committee: Drs. Bell, Foree, and Chipley.

On motion of Dr. Raphael, a committee was appointed to draft a set of by-laws for the Society. Drs. Raphael, Darby, and Drane, committee.

On motion of Dr. Bell, the order of proceedings was so far modified as to permit Dr. Chipley, Chairman of the Committee on Vital Statistics, to make his report. The report is one of great interest, the investigations were thorough, and the writer accompanied the report with a map, by which the relative sanitary conditions of the various counties were designated.

On motion of Dr. Evans, the report was received and referred to the committee on publication.

The Society adjourned to meet at 7½ o'clock, P. M., to hear the address of the President.

According to adjournment, the Society met, and devoted a short time to the business of admitting members, and the nomination of members for honorary membership. Dr. Chipley nominated Prof. E. Bartlett, of New York. Dr. Gross nominated Prof. Drake, of Ohio. And Dr. Yandell nominated Dr. Deaderick, of Tennessee, as honorary members, which were received for future action.

Prof. Miller offered the following resolution, to lie over until next day, for consideration:

Resolved, That the members will faithfully comply with the requisitions of the registration law passed at the last session of the Kentucky Legislature, and do what they can to have it complied with throughout the State.

Dr. Bell moved that Professor Caldwell be invited to meet the Society during its sittings, and participate in its deliberations. Passed unanimously.

The President, Dr. Sutton, then read the annual address, replete with matter of great interest, and with sound advice for elevating the character of the profession. On motion the address was referred to the committee on publication.

The Society adjourned until ten o'clock the next day.

SECOND DAY'S PROCEEDINGS.

Oct. 21—10 o'clock. The Society was called to order by the President. After the reading of the minutes, Dr. Gross moved the formation of the following special committees:

1. On Medical Biography, or the lives of meritorious or distinguished physicians or surgeons of Kentucky.
2. On Medical Literature, or the history of the medical authorship of Kentucky.
3. On the relation between diseases and peculiar geological formations.
4. On the Statistics of Hernia.
5. On the Statistics of Lithotomy and calculous diseases.
6. On the History and mode of management of Hospitals, Asylums, Infirmaries, Penitentiaries, and Prisons.

[This committee was afterwards divided, by assigning the two last named institutions to one committee, the others to another.]

7. On suits for malpractice.
8. On the results of surgical operations in malignant diseases.
9. On Epidemic Erysipelas.
10. On Epidemic Dysentery.
11. On Typhoid Fever.
12. On Placenta Previa.

And, at the suggestion of Dr. Bell, a Committee on the Statistics of Remedies.

Invitations were received from the Louisville University, the Kentucky School of Medicine, the Louisville Marine Hospital, and the Institution for the Blind, requesting the Society to visit those Institutions. All of which were received.

Professor Gross moved that Dr. Drake, of Cincinnati, be invited to take part in the deliberations of the Society.

Dr. D. D. Thompson, on behalf of the physicians of Louisville, invited the Society to a supper at the Louisville Hotel, on Friday night.

Prof. Flint moved that the election of officers should now be made, which was carried. After various ballotings, Dr. Chipley, of Lexington, was declared the choice of the Society for President. Drs. Bell and Drane were appointed a committee to conduct the President to the Chair.

Dr. Peter moved that a committee for nominating the remaining officers be appointed, consisting of one from each county, and one from the city of Louisville. The motion was carried, and the following gentlemen were appointed.

Dr. Darby, of Fayette; Dr. Bell, of Louisville; Dr. Owen, of Henry; Dr. Spillman, of Mercer; Dr. Sutton, of Scott; Dr. Chew of Woodford; Dr. Freeman, of Oldham; Dr. Foree, of Jefferson; Dr. Sneed, of Franklin; Dr. Letcher, of Jessamine.

Dr. Gross moved that the standing committee report in the order in which they stand in the proceedings of last year. Adopted; and on call, several of the committees failed to report. Dr. Evans, of Covington, read an able and well written report on Medical Ethics, which was received and referred to the committee on publication.

Adjourned to three o'clock, P. M.

AFTERNOON SESSION.

Dr. Chipley in the Chair. The society met pursuant to adjournment, and after the election of several members, the nominating committee reported candidates for the various offices; and upon a ballot for each office, the following officers were declared elected:

Dr. E. C. Drane, of Newcastle, Senior Vice President; Dr. J. W. Knight, of Louisville, Junior Vice President; Dr. Sneed, of Frankfort, Recording Secretary; Dr. R. J. Breckenridge, of Louisville, Corresponding Secretary; Dr. D. D. Thompson, of Louisville, Treasurer; Dr. B. Monroe, of Frankfort, Librarian; Drs. Bell, Foree, and Ronald, Committee of Publication.

Dr. Miller, chairman of the Committee on Obstetrics, then read a Report on Chloroform in Midwifery, and the use of the speculum in uterine diseases. The report commanded the profoundest attention; and that portion of it which examined the sentiments of Dr. Lee, of London, contained a triumphant refutation of that gentleman's opinions. The report was received and referred to the committee on publication.

Dr. Sutton read a report on Registration, which was read and referred to the committee on publication.

Dr. Jacobs offered a resolution respecting the clause in the constitution which restricts honorary membership to eminent physicians residing out of Kentucky. Ordered to lie over until the next day.

Adjourned to 7½ o'clock, P. M.

EVENING SESSION.

Dr. Chipley in the Chair. After the election of members, Dr. Wible offered the following preamble and resolution:

WHEREAS, Physicians are frequently called upon to give evidence in courts of justice, to make post mortem examinations, and institute investigations in cases of poisoning, services of a strictly professional character, requiring expense, time, and labor on the part of the physician;

AND WHEREAS, these services are often of great importance to the welfare of society; and as physicians ought not to be expected to perform them without remuneration; it is the opinion of the Kentucky State Medical Society, that the interests of humanity demand that laws be enacted which will properly secure these services when required by coroners and other officers of the law;

Therefore, be it Resolved, That a committee of three be appointed to prepare a report, to be read at the next annual meeting of this Society, on the subject presented in the foregoing preamble.

[We may as well state that Dr. Sutton introduced a resolution for the appointment of a committee "to consider whether any, and if any, what means can be brought into requisition to lessen the heavy burthen of pauper practice, and report at the next annual meeting." The resolution and Dr. Wible's were referred to Dr. Wible, as chairman of the committee, with power to appoint coadjutors.]

Dr. Miller's resolutions, of yesterday, were adopted.

Dr. Darby, as chairman of the committee on Epidemics, read a report for himself, and one from Dr. B. P. Drake; and Dr. Allen, of the same committee, also read a report on Epidemic Dysentery.

The Society then selected Lexington as the place for the next annual meeting.

Adjourned to meet at 10 o'clock to-morrow.

THIRD DAY'S PROCEEDINGS—MORNING SESSION.

Dr. Chipley presiding, Dr. Gross's resolution for special committees was called up and adopted. Dr. Darby reported By-Laws, which report was suspended in order to hear Dr. Gross's report on Sur

gery. This admirable report, one of the finest contributions that has been made to the medical literature of Kentucky, excited the deepest interest; and although expectations were very high, we believe the Professor transcended all anticipations. The report contains the most complete history of Kentucky surgery that has been written, and it would be considered a first class paper among the élogés of the French Institute The report was received and referred to the committee on publication.

Dr. Spillman made a remarkably excellent report on Indigenous Botany, which was referred to the committee on publication.

The following resolutions, appended to the report, were unanimously adopted:

Resolved, That this Society regard the cultivation of our own Botany as essential to a full development of our own professional resources, and to a more successful practice of our art.

Resolved, That the profession throughout the State, and the members of this Society particularly, be requested to give special attention to this subject, and by cultivating the field of observation and research, in connexion with the unexplored regions of vegetable nature within our own limits, ascertain to what extent the demands of the healing art can be supplied at home.

Resolved, That any physician discovering a new remedy, or a new property in one already known, or any information touching the Medical Botany of our State, that can be rendered practically available, be requested to communicate such information to the chairman of the committee on Indigenous Botany.

Resolved, That the committee of Publication, at as early a day as practicable, by a brief circular or otherwise, communicate the objects embraced in these resolutions to the profession throughout the State, giving the name and location of the chairman of the committee on Indigenous Botany, and earnestly requesting contributions.

Dr. Raphael's report on By-Laws was presented, received and adopted. Our limited space forbids the publication of these by-laws in the present number of the Journal.

Dr. Anderson offered a resolution of thanks to Col. Buckner for the use of the Court Room, and attentions to the Society. Unanimously adopted.

Various nominations for membership were acted upon, and the ballot was favorable in all instances.

The following appointments were announced by the President:

STANDING COMMITTEES.

Committee of Arrangements..... Dr. Darby, of Lexington.
" on Practical Medicine.. Prof. Force of Louisville.
" on Improvements in Pharmacy..... Prof. Silliman, "
" on Vital Statistics..... Dr. Sutton, of Georgetown.
" on Obstetrics..... Prof. Powell, of Louisville.
" on Medical Ethics..... Dr. Hewett, "
" on Public Hygiene..... Dr. Bell, "
" on Epidemics..... Prof. Bullitt, "
" on Surgery..... Prof. Flint, "
" on Indigenous Botany... Dr. Emmert, of Pike county.
" on Finance..... Dr. Letcher, of Jessamine.

SPECIAL COMMITTEES.

1. On Medical Biography..... Dr. R. J. Breckenridge, of Louisville.
2. On Medical Literature, &c..... Prof. L. P. Yandell, of Louisville.
3. On Relations between Diseases and particular Geological Formations Prof. Peter, of Lexington.
4. On the Statistics of Hernia..... Dr. S. B. Richardson, of Louisville.
5. On the Statistics of Lithotomy and Calculous diseases..... Prof. Gross, of Louisville.
6. On the History and Management of Hospitals, &c..... Dr. Raphael, "
7. On Penitentiaries and Prisons.... Dr. Sneed, of Frankfort.
8. The results of Surgical Practice in malignant diseases..... Dr. Colescott, of Louisville.
9. On Epidemic Erysipelas..... Dr. Owen, of Newcastle.
10. On Epidemic Dysentery..... Dr. Hynes, of Bardstown.
11. On Typhoid Fever..... Dr. C. H. Fry, of Louisville.
12. Placenta Previa..... Prof. Miller, "
13. On the Statistics of remedies in disease..... Prof. Rogers "

Dr. Thompson, Chairman of Committee on Finance, made a report rom that committee. The report did not include the receipts at the re-

cent meeting. Dr. Bell, Chairman of Committee on Case-Book, reported. He stated the extraordinary labors of Sir W. F. Chambers, of London. That gentleman has been engaged for many years in a most laborious private practice, in the management of a large hospital, and in teaching. He was also very sickly, yet he has kept a minute record of all the phenomena of all his cases, and has filled sixty-seven quarto volumes, each of 400 pages, with records of his cases. These records are all written in Latin. Sir William also made a number of Index books to his quarto volumes, and for years wrote up the hospital books in the style of his private case-books.

The committee urged the absolute necessity of a case-book for every practitioner, in which he may record the symptoms of every case. Two resolutions were appended to the report. The report was referred to the committee on publication, and the resolutions unanimously adopted. The following are the resolutions:

Resolved, That the Kentucky State Medical Society commend to the physicians of the State, and urge upon the members of the Society, the purchase of case-books of the kind described in this report.

Resolved, That in order that the publisher may determine how far he may be justified in putting this case-book to press, that the members present who may wish to encourage this publication, hand their names to the committee on publication, and that the committee shall be authorized to continue to receive these names after the adjournment to the next annual meeting, until a number shall be obtained sufficient to justify the enterprize; and when the committee shall have received this assurance, they shall be authorised to make arrangements with the publisher: Provided, the Society shall not be committed in any way for the expense of publication.

Dr. Raphael offered the following resolutions:

Resolved, That if the amount in the Treasury should not be sufficient to defray the expenses of getting out the proceedings of the Society, that the publishing committee assess the members *pro rata* with such excess.

Resolved, That any member may obtain one copy or additional copies, extra, of the transactions, by paying the cost price of the same.

Resolved, That the President be authorized to fill any vacancies in the delegation to the American Medical Association.

The following delegates were elected: Dr. Chipley, of Lexington, Dr. Freeman of Oldham; Dr. W. C. Sneed, of Frankfort; Dr. Raphael, of Lonisville; Dr. T. S. Bell, of Louisville; Dr. Gore, of Bloomfield; Dr. R. J. Breckenridge, of Louisville, Dr. Moore, of Harrodsburg; Drs. Wible, Hardin, and Hewett, of Louisville.

Prof. Bartlert, of New York; Prof. Drake, of Ohio; and Dr. Deadrick, of Tennessee, were then unanimously elected honorary members.

On motion of Dr. Jacobs, the thanks of the Society were tendered to the officers for the faithful, able, and impartial manner in which they had discharged their respective duties.

The Society then adjourned to meet at Lexington on the third Tuesday in October, 1853.

Thus closed the labors of this Society, and we sincerely believe that the meeting not only did a great service for the Medical profession, but that the good results will continue to show themselve for years to come.

We understand that the volume of Transactions will be put to press immediately; and it will be a book of which the medical men of Kentucky will have reason to be proud. We earnestly hope that every medical man in the State, who feels any interest in his profession, will order a copy of the work. The principal Reports will form a portion of the volume.

We understand, too, that the Committee of Publication are making arrangements with Mr. T. Nelson, of this city, for the immediate publication of the case-book referred to in the proceedings. It will be a proud day for medical science and practice in Kentucky, when all the practitioners of the State commence the use of the case-book. Those who begin it, will find such benefits to themselves and patients, that the practice will not be abandoned speedily. B.

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